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CONTENTS

	Page
Social Medicine	129
Science and Religion. By The Very Reverend W. R. Matthews, K.C.V.O.	130
The Old and the New in India. By Dr. Mulk Raj Anand	131
Past, Present and Future of Aviation. By The Right Hon. Lord Brabazon, P.C.	132
Leukæmia in Animals. By Prof. G. R. Cameron	133
Conditioned Reflexes. By Dr. D. Whitteridge	135
Separation of Isotopes and Thermal Diffusion. By Prof. James Kendall, F.R.S.	136
Future of the Population of Great Britain. By Colonel Sir Charles Arden-Close, K.B.E., C.B., C.M.G., F.R.S.	140
The Human Factor in Production. By Dr. May Smith	142
Absorption and Emission of Radiation in the Atmosphere	144
Obituaries:	
Dr. C. D. Sherborn. By J. R. Norman	146
Mr. R. G. McConnell. By Dr. George Hanson	147
Major G. W. Dunkin	148
News and Views	148
Letters to the Editors:	
Distribution of Attack on Iron or Zinc Partly Immersed in Chloride Solutions.—Dr. Ulick R. Evans	151
Determination of Absolute from Relative X-Ray Intensity Data.—S. H. Yü ; A. J. C. Wilson	151
A New Active Constituent of Pyrethrum Flower.—D. N. Roy and S. M. Ghosh	153
A Recording Paper for Field and General Use.—A. G. Tarrant	153
The Polygene Concept.—Dr. C. D. Darlington, F.R.S.; Prof. R. A. Fisher, F.R.S.	154
Recent Advances in American Archæology	155
Causes of Intertidal Zonation. By Prof. T. A. Stephenson	158
Junction Line Filters	159
'Tack' and Sticky Materials	159

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SOCIAL MEDICINE

THE announcement that the Nuffield Provincial Hospitals Trust is prepared to endow for ten years the creation of a professorship of social medicine in the University of Oxford has created various favourable comments in the general Press. It is one of the signs of the times, which all indicate that the social aspect of medicine is coming inevitably to the fore as the public realization and appreciation of its importance increases.

The War has expedited the change in public opinion indicated by the proposal, but the course of things has been long proceeding. The Elizabethan conformation of the Poor Law meant that no one need starve or lack medical aid in England. It took long centuries to realize the extent of the obligation this imposed, and only recently has its full value been appreciated, behind which is the philosophy of social consciousness which has become more and more sensitive as the decades have succeeded one another. Gradually, voluntary and official hospital provision has grown, including that of special hospitals for particular complaints, and the habit has been created—indeed it is a necessity—of hospital treatment of major diseases. Not only this, the growth of 'specialism' and consequent extension of medical work implied has necessitated special provision, and few people can now deny that they are better treated in these hospitals than in private practice.

The trend is always increasing, and this must of necessity go on, because the standard of effort advancing medical science is always improving. It is to meet this increasing standard of medical attendance that the Nuffield professorship in social medicine now created is especially directed ; it will also have an indirect value in preventing disease, or what in many instances is tantamount to that, in treating it before it reaches an incurable stage. There is by no means enough liaison between the general medical practice of to-day and preventive medicine. Such universal problems as motherhood, childhood and youth, tuberculosis, cancer, venereal diseases should not be left to medical individuals or groups of individuals. Much has been and is being done by various groups (often in collaboration with the Ministry of Health) ; but it is not enough, because only a minor percentage of the population is reached by such methods. Whether the State should take over responsibility is a moot point ; for the grave risk of compulsion is here implied. Compulsory inoculation or treatment for various types of disease might, on casual consideration, seem the easiest way out ; but various inquiries concerning its effects in countries where it exists show that disturbing consequences often arise. In a democracy such as ours, enlightenment and education, especially in healthy living and in medicine as a social science, are clearly the best solutions to a problem of ever-increasing complexity.

This process, which has been increasing in emphasis decade by decade, has been expedited by the War, and many medical practitioners now in military service are unwilling to go back to the hampered

conditions of ordinary single-man medical work. Medicine is clearly developing as a social science and team work is increasing, thus casting off its fetters as a closed profession. As Sir Arthur Newsholme has pointed out*, "in future practice, blundering experiment and erroneous procedures can best be avoided when one knows what others have done and what has been the outcome of their practice".

The procedure of medical insurance is evidence of this process. Already a third of the working-classes, including many black-coated workers, are within the national scheme; and there is concurrence of evidence from the British Medical Association, representing the great mass of the practitioners in Great Britain, of testimony to the fact that this process should be extended to the dependants of insured persons. This would mean that four fifths of the population of the country would be removed from the evils of cash nexus which at present fetters medical work. It is not difficult to see the future course of events. Ere long, under the pressure of the medical profession, all workers will have to be provided for by a national system of medical service. Whether the expenditure incurred will come solely out of rates and taxes, or whether a system of payment by weekly stamps in which all have partnership—employers as well as employed—and in which the State sees that equitable provision is made, is still disputable; but there can be no doubt as to the trend of events in this connexion. Everyone is interested in the maintenance of health; family health and industrial health are merely aspects of the same problem, and everyone is concerned at the present lack of continuity in such medical assistance as is forthcoming. The financial aspects alone of this discontinuity are serious. While not advocating any undue increase in consultant, pathological and X-ray aids to medical practice, their lack is often felt deeply by the general practitioner to the detriment of the patient. In particular, the fastidious general practitioner often leaves off attending his patient when in more favourable circumstances he would like to continue to watch him. This consequence of discontinuous medical care is to the detriment of both medical man and patient.

For this state of things the growth of medical science is largely responsible. It is no one's fault, but the present condition of things is becoming intolerable, and there is an increasing call for the availability of expert medical help to the full extent needed.

It has been increasingly clear that there is need for a closer unification of local and national medical services, and few will doubt that such unification should not be left to medical men, but should be organized by the public authorities in consultation with the former. This will be one of the urgent immediate problems in medicine viewed as a social science.

The Nuffield Trust endowment for this and other purposes will facilitate matters. One may rightly wish God-speed to its efforts, and special success to the new professor of social medicine.

* Medicine and the State. By Sir Arthur Newsholme. (London: George Allen and Unwin, Ltd., 1932.) 7s. 6d. net.

SCIENCE AND RELIGION

The Human Meaning of Science

By Arthur H. Compton. (John Calvin McNair Lectures.) Pp. xiv+88. (Chapel Hill, N.C.: University of North Carolina Press; London: Oxford University Press, 1940.) 6s.

THIS is a small book on a very large subject, and it would be unreasonable to expect any comprehensive or fundamental discussion in three lectures. What the author gives us is interesting enough, though the reader must often find himself asking questions which in these pages at least have no answer. The second lecture, which is on the subject of human freedom and physical law, is a lucid and valuable statement of an argument for freedom. Dr. Compton does not allege that modern developments of physics can be used as an argument to establish the reality of freedom, but he does hold, and surely with good ground, that the argument in favour of determinism which was based on the alleged universality of natural law no longer possesses its ancient cogency. The newer quantum mechanics are "consistent with the postulate of freedom". There is no discussion of the meaning of freedom, but it appears to be identified with the "effectiveness of purpose" as in the following sentence: "My experience of the effectiveness of purpose is more direct and cogent than any logical argument based upon scientific generalizations".

The first lecture in the book is on "Science, Religion and Human Growth", and it must be confessed that to one reader at least it has seemed to err on the side of optimism. We are taken rapidly through the obvious advantages which science has conferred on the human race, and stress is laid on the mental and spiritual gifts of science. We have been taught "the importance of reliable knowledge". We have "gained confidence in our strength" and have therefore a more hopeful outlook on the future than our unscientific ancestors. One cannot help wondering whether these statements are accurate. It may be that we understand the importance of reliable knowledge, but the fact is that there never was a period when men were more easily swayed by propaganda, which is usually the dissemination of unreliable knowledge, nor does a contemplation of the possibilities of scientific warfare conduce in all minds to a hopeful view of the prospects for civilization.

There is, however, one consequence of scientific civilization as defined by Dr. Compton about which his language is ambiguous. "It would seem to be a corollory of the growing complexity of civilization that people must give more and more attention to the rights of others." The word "must" is notoriously misleading, and it is not clear whether the author means that science and technology produce altruism or whether the progress of science and technology ought to be accompanied by a growth of conscience. On the whole, Dr. Compton seems to believe that law and justice not only ought to prevail but necessarily will prevail. We may at least agree that unless co-operation is achieved and placed upon firm foundations, the conditions that make advance in science possible will eventually disappear. No doubt this would be most deplorable, but it is not evident that it could not happen.

The third lecture, on "The Need for God in an Age of Science", would require a long discussion to do justice to its argument. The thesis is that "the

hypothesis of an intelligent God gives a more reasonable interpretation of the world than any other", and the chief emphasis is laid upon the evidence of "direction" or of intelligence in evolution. "Evolution is in itself no explanation of these remarkable processes; the theory of evolution is merely the description of how the processes occur. It is, however, difficult to avoid an interpretation which sees underlying these evolutionary processes an intelligent power, directing them with some great purpose." I have, naturally, no quarrel with this conclusion, but I am moved once again by reading this stimulating little book to wish that I knew precisely what was meant by the "theory of evolution".

W. R. MATTHEWS.

THE OLD AND THE NEW IN INDIA

Ethical Ideals in India To-day

Conway Memorial Lecture delivered at Conway Hall, Red Lion Square, W.C.1, on March 22, 1942. By Dr. Edward Thompson. Pp. vi+39. (London: Watts and Co., Ltd., 1942.) Cloth, 2*s*. net; paper, 1*s*. net.

THERE is a certain appropriateness in contemplating the development of ethical ideas in contemporary India, for there are few countries, Russia and China excluded, where the march of history has engendered such violent conflicts in the mental and moral attitudes of peoples as in Hindustan. Dr. Edward Thompson, who has devoted many years of service to India, did well, therefore, in choosing this theme for his Conway Memorial lecture this year.

The main thing about recent ethical ideas in India, which emerges from Dr. Thompson's exposition, is that they are much more intimately allied to the modern political movement than ethical ideas have been in Europe. So that they would seem, at first sight, to be in the main tradition of Indian thought, which looks at man as a whole, and in which man's philosophical, ethical, æsthetic and political ideas have been regarded as all part and parcel of each other. Yet, these contemporary ethical ideas in India, being more intimately associated with the individual great men of whom Dr. Thompson speaks in his lecture than with schools of thought and religion, partake of the character of the modern European, individualist sense of values.

This peculiarity is implicit in the workings of India's recent history. The feudal structure of society broke down in India in the last hundred years. The old self-governing, self-sufficient village system was smashed and the locomotive connected the most obscure corners of the land to the town, the seaport and the world market. "Telegrams and anger" came into India and the "steel frame" of a central government introduced a political and economic system which changed the very fabric of old social relations. The capricious and fluctuating prices of grain in the exchanges of the world destroyed the uncanny, unknown 'fate', symbolized in the all-powerful Nature, and substituted a new unseen fate, the stockbroker. All the ancient cultural traditions, though surviving as quasi-feudal forms in the very midst of this modernity, began to corrode from within.

In such an India, in the midst of these anachronisms, the difficulties of regeneration having been increased by the frustrations of political advance, the very best minds of India struggled to adapt themselves and their country to the outside world, which itself was changing fast. The shocks upon their sensibilities were acute and so were their reactions.

From Ram Mohan Roy, the first great modern Indian who believed in the truth of the unity of all mankind, to Jawaharlal Nehru, a believer in international socialism, there have been many vital and original responses to the complex questions of our age. Dr. Thompson chooses for special attention the answers of three significant men: Gandhi, Tagore and Iqbal. All three, ranking high among world thinkers, are shown to be oscillating between the old and the new in contemporary India, torn almost like that great Englishman whom Shakespeare pictured as the Prince of Denmark.

What exactly was the problem they posed before themselves, and how did they seek to solve it? I have already referred to the great social upheaval, the overturning that happened in India after the coming of the British. That was the atmosphere in which these three men grew up, and, inevitably, they asked themselves to what extent they could actively help or hinder this overturning, and what was the ethics of behaviour called forth from them.

Gandhi read Ruskin's "Unto This Last". Perhaps he also heard "the shrill cry that goes up from the factory chimneys, fiercer than the furnace blasts". Anyhow, he determined to change his life in the light of that book, or rather in the light of the three principles he deduced from it: (1) the good of the individual is contained in the good of all; (2) the dignity of labour; (3) the life of the tiller of the soil and handicraftsman is to be preferred to all. After serving in an ambulance unit in the Boer War, he added to this the belief in *Brahmcharya*, or sexual continence. Further, since he had become aware of the restrictions placed on Indians by the South African Government, he elaborated a further doctrine called *Satyagraha*, or soul-force, with which he sought to struggle against authority, the actual method being the old Indian practice of *Dharna*, the aggrieved sitting at the doorstep of the oppresser until his grievances were remedied. This 'stay-put' strike, backed by soul-force or truth-force, later known as civil disobedience, was to be characterized by a strict *ahimsa*, which Gandhi translated as non-violence, and which has become familiar in our time through its adoption in the mass campaigns of the Indian National Congress in its struggle for freedom.

Dr. Thompson's exposition of Tagore's ethical beliefs is also an implied criticism of Gandhi's doctrine. For Tagore, though aware of the enormous gains achieved by non-violence, considered it to be ethically an inadequate doctrine. "I believe," he wrote in an open letter to Gandhi, "in the efficacy of *ahimsa* as the means of overcoming the congregated might of physical force on which the political power in all countries mainly rests. But like every other moral principle, *ahimsa* has to spring from the depth of mind, and it must not be forced upon man from some outside appeal of urgent need. . . . When it concerns an immense multitude of men of different traditions and stages of culture, and when the object for which such repression is exercised needs a prolonged period of struggle, complex in character, I cannot think it possible of attainment."

Tagore himself, "choked for breath in the hideous nightmare of our present time", tried to be an

individual and to teach others to be individuals in his school at Shantiniketan. He regarded with contempt the 'mendicancy' of Gandhi's method and held the more positive belief that "weakness is sinful, because it is a menace to the strong and the surest cause of downfall for others than those who own it." This individualism, which owed a great deal to the West, alternated with many, even anthropomorphic, moods in him before it reached a kind of scientific humanism towards the end of his life. Then Tagore was inclined to believe in a new morality of the poor such as may arise from a change in the *status quo*, as a reaction against the prevalent morality in which the rich can steal millions but the poor are imprisoned for stealing a loaf of bread, a morality which, he believed, was extraneous to society and had lost touch with it.

The approach of the North Indian poet, Muhammad Iqbal, shares the positive urge of Tagore, though, being more concerned to re-interpret Islamic thought in order to supply the lacunæ in the theoretical background of modern Muhammadan communities, he found himself adopting the old ethics of the Koran. Nothing less would do than the formation of a holy kingdom, *Pakistan*, somewhere in the north of India, where the old brotherhood of Islam would be revived. This brotherhood would not be liberal, nor based on the recognition of human or ethical equality of men, but on the strong individual re-orientating, or rather stretching, the old gospel to admit of modern scientific ideas such as evolution, God as time, the final judgment day as an alternation of day and night when good deeds bring their own reward, as in the Hindu doctrine of *Karma*.

Except for Tagore and Nehru, both imbued with the atmosphere of our age and straining for a new morality, the great leaders of Indian thought fail in the final analysis and yield to the past. Rising on the crest of an emancipatory movement, the children of a great overturning in Indian history, they resent further change. The resistance to the machine in Gandhi's ethics is reminiscent not only of Ruskin but also of the illustrious Tolstoy, the man who greeted the abolition of serfdom only to cry halt in front of the English industrial revolution.

Why does this peaceful Yasanya Polyana ethic reappear in more or less the same form at the ashrams of Sabramati and Wardha? Dr. Thompson has a brilliant flash of insight into such recurrences when he compares Gandhi's recommendation of non-violence to the Abyssinians against Mussolini, with Chamberlain's appeasement of Hitler. In fact, the whole lecture shows an uncannily sympathetic grasp of the Indian attitudes in their interconnexion with world modes of thought. I have not the space to bring out the significance of all the attitudes Dr. Thompson treats of, but I may answer the question I have just raised.

It is possible that non-violence springs from a feeling of weakness in the face of the tiger; it is conceivable that it owes a great deal to the lack of cohesion in the forces behind one; but it is certain that it springs from an almost unconscious acquiescence to those who seek to preserve the past, even in putrescent decay, and who dread another over turning. Because non-violence in India implies active moral resistance, it is certainly more positive than appeasement, but any dynamism it has is due to its organized application in mass campaigns rather than to the individual who conceived it, however high the ethical merit of the conception—the proof of the pudding, like that of any other, is in the eating. We shall see how non-violence fares in its application against the marauding hordes of Nippon now ranged on India's frontiers, if the Government still ties India's hands and if Gandhi still insists on applying it.

MULK RAJ ANAND.

PAST, PRESENT AND FUTURE OF AVIATION

Air Power and Civilization

By M. J. Bernard Davy. Pp. viii+202+8 plates. (London: George Allen and Unwin, Ltd., 1941.) 8*s*. 6*d*. net.

MR. DAVY'S book merits the attention of many people, for it is comprehensive. Those who are interested in the days previous to the invention of flight will find some very interesting chapters going back to the ideas of Leonardo da Vinci and tracing the subject through the great balloons of Montgolfier to the advent of the aeroplane. Then Mr. Davy deals with the historical production of flight, and here nobody has greater authority. I myself am very critical of the contributions by different people which have been very much mixed up lately with fallacious claims. Mr. Davy, with his knowledge and study of these early times, brought about no doubt by his association with the aeronautical exhibit in the Science Museum, gets everything right, so that I look upon his early historical side as something well worth preserving and referring to, should reference back be sought on any point.

The book then passes to the power of the aeroplane, describing with great interest the first aerial attack and the use of the aeroplane as a lethal weapon, culminating in the present situation, and then it deals with the future. It will be seen, therefore, that there is little in aviation, approached in a general way, that Mr. Davy does not deal with; and for that reason his book must command the respect and attention of all students of this great problem.

Mr. Davy does not think that internationalization of air is the complete solution, or that it could be feasible. I cannot agree entirely with this. I think so much depends on at what time it is done. It could be done now, but if it is put off until it is too late it will never be done, and the continuance of subsidized air lines as cover for air forces will start again. Mr. Davy thinks that regional air police forces might be a beginning, eventually coming all together in a world federation. It is a matter of how much we can do. No one will dispute that a world federation of the air is imperative. Whether it can be done all at one time, or whether the idea of regional air police forces does not lend itself to further conflict, is for the reader to decide. But here is an attempt to deal with this great question of air control after the War, and the more people are conversant with the ideals and the difficulties the sooner we may reach some solution of this difficult problem.

Mr. Davy does not direct attention to what, I think, is rather a curious situation in flight. There are many things we use to-day which were not possible for man to have created—say two or three hundred years ago—because the technique in manufacture was not available to him. For example, the motor-car could never have been built for the Romans; they had neither lathes, the metallurgical knowledge, nor the engineering ability. But look

to-day at a modern high-efficiency glider—a glider with a rate of fall of 1 in 20; there is nothing in it that could not have been built a thousand years ago with ease. So that although actual power flight waited on the internal combustion engine, had gliding been indulged in seriously and the basic principles of gliding flight appreciated, we could indeed, a thousand years ago and more, have had controlled gliding flight with its very vast potentialities already available to mankind before the advent of a power unit. In fact, I think, gliding flight is one of those things which came very much later in history than was possible.

Mr. Davy says that Nature did not provide any devised pattern for man to copy, but I think for gliding flight with those very high aspect ratio birds such as the gull, there was indeed a pattern to be copied, and although propulsion by the flap wing introduced very grave mechanical difficulties, yet the gull is so lazy and uses air currents with such efficiency that it did not require very great intelligence to realize that the trouble could have partially been got over by using rising currents of air apart from propulsion. Even to-day we have a lot to learn from Nature, especially in noticing that maximum power in the bird is used not only for getting off, but in landing, whereas all our aeroplanes land on the straight dead glide—a system which is not utilized in Nature except occasionally on the water. Usually tremendous power is exerted to stop forward movement and give lift just before contact with the ground. Something along these lines is badly wanted to-day, as all aircraft are in the greatest danger when landing.

Due tribute is paid to Caley, Henson and Stringfellow, which they well deserve. Stringfellow, I see, gave performances of a model in Cremorne Gardens in 1848. This remarkable seat of aviation and entertainment, now filled by Lot's Road power station, must have been a very attractive rendezvous. Some of the first ascents by balloon were made from these Gardens.

The work of Pilcher has not been given enough attention in the book. He was not only a follower of Lilienthal in gliding, but he himself also invented some very remarkable devices. For example, he was towed up into the air as a kite by a system of tackle, gearing himself up and being pulled by a horse. Along these lines he did exactly what is to-day done by a motor-car pulling a glider up as a kite to start with before it glides. Lilienthal never did that, always relying on downward flight in a rising column of air on the surface of a hill.

I do not think the Wrights' patent is put in the clearest way. This patent was the claim that the warping of the wing or the aileron did not restore lateral stability and that if you wished to restore it, it was not only necessary to put the extra angle on the wing, but it was also necessary to interconnect this with rudder action of a reverse type. Otherwise the extra drag due to the increased angle of the wing rendered nugatory the potential extra lifting power. Mr. Davy deals with the Wright-Langley dispute in a very interesting part of his book. It was a situation which is little appreciated in Great Britain.

In the early heroic epoch of flight in Europe, not enough tribute is paid to the first Gnome engine, which was a revolutionary design. It was self-cooling by the rotation of the cylinders, and for its day was extremely reliable. Another interesting point is that due to the exhaust valve in the top of the cylinder, the engine consumed more lubricating oil than petrol, and it was not until somebody remembered the extraordinary properties of castor oil, which becomes stickier the hotter it gets, that this engine became a practical proposition. This engine did more to help along European aviation than any other factor.

I have told the story before of how on behalf of the Aero Club I went on a deputation to the War Office offering to lend two machines for the manœuvres and was rebuffed by them saying that they did not see any uses for aircraft in war. I always consider this a very remarkable episode in my life, but Davy quotes an equally remarkable incident, which is that "in 1907 the Wright brothers corresponded with the First Lord of the Admiralty concerning their invention, and they were informed by him that, after consulting expert advisers, the Board of Admiralty 'were of opinion that they [aeroplanes] would not be of any practical use to the Naval Service' ". It sounds amusing now, but the suspicion rather creeps into one's mind as to whether the same thing is not being done to-day relative to new inventions.

I am glad the bombing of Rotterdam on May 14, 1940, is brought into the book, because here I think is one of the most dastardly air attacks that was ever made or ever will be made. Fifty thousand casualties, including thirty thousand dead, against the unoffending Dutch is a blot upon the *Luftwaffe* that can never be extinguished.

I am pleased to find that Mr. Davy accentuates the point that real progress on beneficial lines will not really occur if the design and manufacture of aeroplanes is to be determined by the requirements of naval and military air forces. That has been one of the reasons more than any other that progress has not been made along real civilizing lines. There is much to be done—assisted take-off, high wing loading, non-inflammable fuel, better landings, pressurized cabins, jet propulsion; these are all problems of the future which will be great contributions towards flying as a safe and prosperous form of transportation.

Mr. Davy deserves great credit for his book. Not only will the reading of it convey immense knowledge of a subject which from the practical point of view is only some thirty years old, but also it must leave in the minds of all readers food for contemplation and thought as to the future. If I have any criticism, it is that the illustrations might have been better selected, but I am glad to see included the historical picture of the first flight on December 17, 1903.

BRABAZON OF TARA.

LEUKÆMIA IN ANIMALS

Spontaneous and Experimental Leukæmia in Animals

By Dr. Julius Engelbreth-Holm. Pp. xxiii+245+24 plates. (Edinburgh and London: Oliver and Boyd, 1942.) 15*s*. net.

LEUKÆMIA, though a relatively rare disease, has many appeals. The clinician is confronted with a progressive and mysterious increase in the white cells of the blood and blood-forming tissues, resulting, unhappily, in the loss of young life. For the hæmatologist, the chief problem is the origin of these blood cells; the specialist in cancer research is concerned since there are many features in common between leukæmia and malignant tumours. The student of comparative disease, the virus investigator and the

geneticist find fascinating problems, while the theoretical pathologist may well regard leukæmia as a substantial prop to the theory of cellular pathology which was clearly enunciated by Rudolph Virchow (1858), himself one of the foremost investigators of human leukæmia.

Leukæmic conditions are found with some frequency in birds and mammals. Fowl leukæmia is most often a disease of the lymphocytes, whether they are circulating in the blood stream or confined to certain regions such as the lymph glands, spleen or bone marrow. There is a close resemblance between these lymphogenous leukæmias and the economically important 'fowl paralysis' (neuro-lymphomatosis). Curious seasonal variations are seen with fowl leukæmia, reminiscent of the seasonal variations in rate of growth, fatality and transmissibility of certain fowl tumours (sarcomata) induced by carcinogenic agents (Peacock, 1935).

Spontaneous leukæmia is also found in adult dogs, pigs, cattle and rodents. In the mouse it is a common disease occurring in forms very like those known in human beings, so that mouse leukæmias have been intensively studied. Various kinds of white blood cells are affected and enormous numbers of such newly formed cells appear in the blood, while the organs in which these cells are normally congregated (lymph glands, spleen, bone marrow and to a lesser extent the liver) may be greatly enlarged from massive deposits of abnormal cells. On the whole, birds and mammals show an absolute preponderance of leukæmias in which the lymphocyte is the chief cell affected.

Among the many varieties of fowl leukæmia are some which can be transmitted to healthy birds by cell-free centrifugates and filtrates (Ellermann and Bang, 1908). Transmission succeeds with remarkably small doses. Furth (1932) found that as little as 0·000001 c.c.m. fowl plasma could transmit the disease. Chickens a few days old are more susceptible than adult fowls, and seasonal variations in 'takes' are known. A filterable virus is naturally enough thought of as the cause. There is evidence that the virus of fowl leukæmia is not strictly species specific, for it can be transmitted to guinea fowls, pheasants and turkeys. When introduced into the blood of susceptible birds, the virus disappears from the blood vessels for a time, localizing in the bone-marrow and possibly spleen and liver, where it induces proliferation of blood cells. The virus is about 65–75 mμ in size, is thermo-labile, survives prolonged drying, is destroyed by oxygenation, but resists irradiation. It can be cultivated *in vitro* only in the presence of cells. Thus it has many resemblances to the causative virus of the Rous sarcoma of fowls. There is evidence of natural immunity in some birds and active immunization has succeeded.

Transmission of leukæmia is also possible in rodents. In non-inbred strains of mice, transmission succeeds in animals of similar genetic constitution. In mixed strains, transmission fails unless resistance is lowered by X-ray exposure. Inbred strains can be produced with a high incidence of spontaneous leukæmia. The disease can be transmitted only by means of living cells, no cell-free virus having been demonstrated. A single cell injected intravenously may suffice to transmit the disease (Furth and Kahn, 1937). Dispersion and multiplication of such cells, apparently without co-operation of the host's tissues, is responsible for the disease. There is, however, some evidence that metabolic changes, somewhat like those seen in malignancy, occur in lymph glands of leukæmic strains before morphological signs of leukæmia can be recognized (Victor and Potter, 1935), but much more work is required on this obscure subject.

The importance of heredity in spontaneous animal leukæmias has been extensively studied, especially in mice. It would appear that there is a maternal, non-chromosomal factor which differs from the milk-transmitted factor demonstrated by Bittner (1939) in the case of cancer of the breast in some mouse strains. Hereditary factors play an important part in susceptibility to transmitted leukæmia, but the genetic constitution of the tissue inoculated complicates the experiment. Mice of an inbred strain susceptible to inoculation with tissue from one line may thus be resistant to leukæmic tissue from another line.

It has been claimed at various times that experimental leukæmia can be produced by the action of carcinogenic substances, just as sarcomata are produced by these. Striking blood changes do occur in such circumstances, but they are not wholly convincing of leukæmia and are seldom accompanied by blood-cell infiltration of the tissues. Investigators have preferred to speak of them as leukæmoid changes, reserving for the future the right to change their views as to the true nature of such conditions. In mice of known genetic constitution showing a very low incidence of spontaneous leukæmia the condition can be produced by carcinogenic agents. Thus in the Aka strain in which 50–70 per cent of animals more than eight months old develop lymphogenous leukæmia, carcinogens induce sarcoma and carcinoma as well as leukæmia in mice less than five months old (Engelbreth-Holm, 1940). Such treatment seems to accelerate both tumour and leukæmia production. The incidence of leukæmia can also be increased in mixed stocks and purer strains by irradiation with X-rays or by œstrogens.

It is not surprising, therefore, that many investigators now accept Ribbert's (1904) view that leukæmias in animals are neoplastic in nature, but whether they are truly malignant or benign is not certain. There is no clear-cut border-line, however, between lymphogenous leukæmia and lymphosarcoma in mice. Leukæmic disease, as with new growths, commences as a local change and becomes generalized, any differences being explained as consequences of the special relationship of leukæmias to the vascular system. Since there are so many clinical and pathological analogies between the animal and human leukæmias it is obvious that investigations into their relationship to viruses, transmissibility and genetic constitution hold out a good deal of promise for the future, and man may once again be indebted to his humbler relatives for the solution of a baffling problem.

Dr. Engelbreth-Holm, director of the Cancer Research Laboratory of the Danish Anti-Cancer League and himself an original worker on animal leukæmias, is to be congratulated on a fascinating book written at the invitation of the Scientific Advisory Committee of the Lady Tata Memorial Trust. Not only research workers in this very specialized field of blood diseases but also all who have an interest in the progress of experimental medicine will enjoy the clear presentation of a variety of facts drawn from a widely dispersed literature. Special commendation is due to the translator of the Danish manuscript, the editors of the English version and the librarian of the National Institute for Medical Research, London, who checked the references, for their labours.

G. R. CAMERON.

CONDITIONED REFLEXES

Lectures on Conditioned Reflexes

Vol. 2: Conditioned Reflexes and Psychiatry. By Prof. Ivan Petrovitch Pavlov. Translated and edited by Dr. W. Horsley Gantt. Pp. 199+7 plates. (London: Lawrence and Wishart, Ltd., 1941.) 8*s*. 6*d*. net.

ONE is often grateful for the existence of general surveys which are written for international congresses or commemorative volumes. They provide excellent opportunities of summarizing a subject in a form which gracefully supplies entertainment and instruction. It is very doubtful, however, if there is any justification for the collection and republication of the general reviews written by a single author. Inevitably they cover large areas of the subject, and inevitably they overlap. Brief accounts of the same illustrative experiment reappear time after time, until one longs for tables of data and experimental protocols. Of the fifteen chapters in this volume, eleven consist of general articles. The translator has carried out extensive condensation and excision, but far too much that is redundant remains.

The last chapter is by far the best. It was written in 1935, the year before Pavlov's death, and in it he has summarized in twenty pages the whole of his work on conditioned reflexes. It is the most complete and orderly presentation in the book. The other outstanding chapter appeared in the *Psychological Review* in 1932, translated by Dr. R. S. Lyman. Its style is clear and vigorous, and the subject-matter is scientific polemic at its best. Pavlov defends himself against Guthrie and Lashley by a masterly restatement of his own position, and his criticism of Lashley is fair and penetrating. His comments on experiments on maze-running exemplify his approach to a problem and his instinct for profitable analysis.

The remainder of the book is disappointing. The translation is obscure and clumsy, and there are no references. The introduction contains a biographical sketch of Pavlov's later years, and a section entitled "A Critical Evaluation of Pavlov's Work". This consists of a hotchpotch of the considered and unconsidered opinions of physiologists and psychologists, and a remark addressed to the author by Bernard Shaw at Lady Astor's.

Since 1927, Pavlov and his school seem to have turned their attention chiefly to the study of the disturbances of behaviour, which he calls experimental neuroses. The methods used include the application of very strong stimuli, the protracted use of inhibitory stimuli, the presentation of positive and inhibitory stimuli in quick succession, and castration. On the basis of their behaviour in these conditions, Pavlov divides his dogs into choleric, sanguine, phlegmatic and melancholic types. This classification depends on incompletely defined concepts of 'strong' and 'weak' nervous systems, but it is claimed that it is of use in the production of neuroses at will, and in their cure by appropriate treatment.

When he was more than seventy, Pavlov plunged into the study of human psychiatry. He began to attend psychiatric clinics, and re-opened the writings of the clinical psychiatrists. Naturally, he began to interpret these new phenomena in more familiar terms. In these lectures, analogies abound between human neurasthenia and experimental neuroses, between schizophrenia and chronic hypnosis in dogs. Paranoia and obsessional neuroses are said to be manifestations of pathological inertia of points of the cortex. The classification of men into artists and thinkers, and the conception of words as conditioned stimuli of a second order are further examples of Pavlov's passion for simple hypotheses with which to try to reduce chaos to order.

It is a pity that Pavlov did not begin by trying to verify in man his conclusions from animal experiment. Instead, he writes as one bringing light to the blind. In 1927, he began the last chapter of "Conditioned Reflexes" with a firm warning against the premature application of "these first steps in elucidating the physiology of the cortex . . . to the intricate problems of the higher psychic activities in man, when at the present stage of our work, no detailed application of its results to man is yet permissible". It is difficult to feel that the subsequent experimental work described here has provided the evangel that alone could justify Pavlov's disregard of his own warning. He even supplies evidence himself that his results on the dog are not, in fact, generally applicable. Speaking of the spread of prolonged inhibition over the hemispheres in a dog, he remarks that "this has been shown from the experiments by Voskresensky on monkeys, to be a special property of the dog's nervous system". In spite of this, we find that this spread of inhibition over the cortex is invoked to explain sleep, hypnosis, and the states of hebephrenia and catatonia in man.

The main difficulty in Pavlov's work, the extreme complexity of conditions from which the stimulus had to be isolated, was brilliantly overcome. The second major difficulty, the degree to which experimental conditions interfered with normal cortical function, he at least recognized: "Hypnotization begins when the dog enters the experimental room, sometimes even before he gets on the stand, increasing as the experiment progresses". The insuperable difficulty was that Pavlov was limited to external observation of stimulus and response. He could trace roughly the anatomical pathways involved, but could know nothing of the intermediate processes. Studies on the whole animal must always be interpreted in the light of analytical experiments, and in the absence of such knowledge it is not surprising that Pavlov has bridged the gap between stimulus and response by a mass of hypotheses. Sometimes, by varying his conditions, he has been able to weed out false hypotheses, but often they are left to buttress one another. Attempts by others to re-interpret his data have not been more successful. Denny Brown's substitution of 'attention' and 'distraction' for Pavlov's 'irradiation' and 'waves of inhibition' seems to depend on personal preference, and the great advances in our knowledge of the behaviour of the spinal cord in the last ten years have made interpretations based on classical cord physiology look a little premature.

With the development of a real physiology of the cortex by Adrian, Bremer and Dusser de Barenne, a surer basis for interpretation is being built up. It is already possible to put some of Pavlov's broader hypotheses to experimental test by electrical methods in the intact animal. Studies of cerebral activity in animals will for long be based on Pavlov's work, and the English-speaking worker will turn first to Anrep's translation of "Conditioned Reflexes". If the present volume induces new students of cortical physiology to turn back to that great work, it will have been of lasting value.

D. WHITTERIDGE.

SEPARATION OF ISOTOPES AND THERMAL DIFFUSION*

By PROF. JAMES KENDALL, F.R.S.
University of Edinburgh

THE fact that the same element may exist in two or more forms, which are chemically identical even though their atomic weights differ, was first noted by Soddy (1910) in connexion with radioactive elements. A little later Sir J. J. Thomson (1913) showed, by the method of positive ray analysis, that neon, with an average atomic weight of 20·2, is a mixture of two kinds of atoms with masses 20 and 22 respectively. The brilliant extension of this method of attack by Aston established that most of the common elements are similarly inhomogeneous. Chlorine (35·457) is a mixture of ^{35}Cl and ^{37}Cl; tin (118·70) exists in as many as ten forms, with atomic masses ranging from 112 to 124. These chemically identical species of the same element with diverse masses are known as *isotopes*.

The separation of isotopes has been strenuously attempted by many chemists during the last twenty-five years. There is only one instance where Nature has, in a sense, performed this separation for us—lead derived from uranium disintegrations consists mainly of the isotope with mass 206, while lead derived from thorium disintegrations contains chiefly the isotope with mass 208. In all other cases the proportions of the various isotopes of any particular element appear to be practically constant, whatever the source. Partial separation has proved possible, although exceedingly difficult, in several investigations; complete separation was first achieved in 1933 with hydrogen.

This lecture summarizes, in the main, a series of researches in this field which I and my co-workers have carried out since 1919.

Ionic Mobility

A long-standing controversy has been waged on ionic mobility; one school insisting that ionic mobility is fundamentally dependent upon ionic volume, another being equally confident that it is fundamentally dependent upon ionic mass. The results available in the literature for homologous series of organic anions and cations have been utilized by both parties to prove their respective points; but, since we have no definite knowledge as to what amount of solvent accompanies any ion in its journey towards an electrode, and since it is the *total mass* or *total volume* of the ion and of its accompanying solvent envelope which must be taken into account, such data obviously offer us no means for definitely determining the problem.

The discovery that isotopes possess equal atomic volumes first put us in a position to impose a crucial test, for isotopic ions necessarily *differ in mass*. If mass is influential, therefore, it should be possible to obtain a separation of isotopes by taking advantage of the fact that the lighter ion will migrate more rapidly than the heavier. This idea of an 'isotopic race', however, cannot be carried out experimentally as simply as it might seem at first sight. Ions do not compete under the influence of the electric current in the manner of a track meet, unless we extend our experiences to imagine a continuous relay race. We cannot start all of our ions at one point and obtain a separation by noting when those of a certain species have passed a given goal, for there must be maintained a steady supply all the way from one electrode to another in order for the current to pass, and it will not help us much if a faster ion hurries ahead of its slower neighbour, since it will merely find itself in the company of other slower ions which happened to start a little in advance. By a modification of the experimental procedure, nevertheless, our 'isotopic race' may be converted into a 'parade' which can be suitably regulated.

The apparatus used is shown in the accompanying diagram, and its applicability may be illustrated by a condensed description of the technique employed in the case of chlorine.

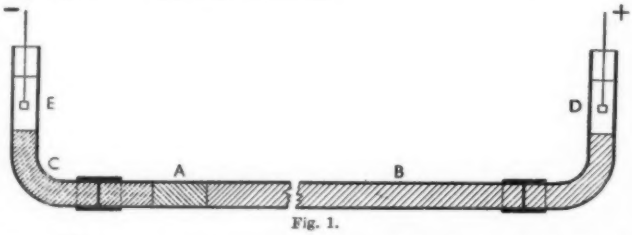

Fig. 1.

An agar-agar gel *A* containing sodium chloride is inserted as a short middle section in a long horizontal tube of Pyrex glass, one and a half inches in internal diameter. On one side of the chloride gel is added a gel *B* containing sodium hydroxide; on the other side a gel *C* containing sodium acetate. The ends of the tube are connected with right-angled Pyrex bends of the same diameter, and the gels continue well up into these bends, as in the diagram. Above the hydroxide gel, after it has set, is poured concentrated sodium hydroxide solution *D*, and above the acetate gel a solution *E* of sodium acetate in concentrated acetic acid. Platinum electrodes are placed in these two solutions, and a current is passed through the tube, the electrode in *D* being made the anode and the electrode in *E* the cathode. The heating effect of the current upon the gel may be minimized by submerging the tube in a long trough filled with running tap-water.

At the beginning of the run, the boundary surfaces between the various sections of the gel are perfectly sharp. When the current is turned on, the boundaries move towards the anode. Inasmuch as there is a faster-moving ion in front of the chloride ion, and a slower-moving ion behind it, admixture of the salts is absolutely prohibited and the boundaries remain distinct throughout the whole experiment. In order to maintain the width of the chloride section approximately constant, it is well to arrange the concentrations of the various salts in their respective gels in accordance with the transference numbers of their anions. Even if this is not done exactly, however, the boundary concentrations soon automatically adjust themselves to the required ratios.

The rate at which the boundaries move depends upon the potential drop between the electrodes, the length of the tube, and the concentrations of the solutions. In actual practice, the horizontal tube is made up of several three-foot sections connected by rubber bands, and the current is regulated (100–500 volts) so that the boundaries advance about 12–18 in.

* From a lecture at the Royal Institution, delivered on May 8.

a day. When the front chloride boundary has almost reached the end of the tube, the apparatus is taken apart. The two rear sections are discarded, two new sections filled with hydroxide gel are inserted in front of the chloride, and new bends are fitted on as before. The chloride ions are now forced to migrate into these two new sections, and the whole procedure is repeated until they have progressed through about 100 ft. of the gel. The chloride gel is then removed from the tube and immediately cut up into strips about 1 cm. in width.

If, now, the isotopic chloride ions with atomic masses 35 and 37 respectively possess significantly different mobilities, the front segments of the gel will contain only the faster-moving and the rear segments only the slower-moving isotope. Preliminary experiments in which a mixture of two sodium salts with anions of known mobilities was substituted for the middle sodium chloride section fully demonstrated this point. Thus, when a mixture of sodium iodide and sodium thiocyanate was employed and the boundary was moved only a few feet, the front segments were found to contain only sodium iodide and the rear segments only sodium thiocyanate. The observed lag of the slower ion was almost exactly equal to that calculated from the difference in mobilities. In this particular case the difference is approximately 16 per cent. In subsequent experiments this difference was narrowed by choosing other suitable pairs of ions: for example, barium and calcium, barium and strontium, and iodide and chloride. The differences in mobilities are here reduced to 8 per cent, 5 per cent and 1 per cent respectively, and the results obtained showed that the method could be depended upon to provide a very considerable degree of separation even at the lowest limit tested.

It was highly disappointing, therefore, to be forced to admit from the results of all our experiments with isotopic ions that no significant separation could ever be detected. The only conclusion that can logically be drawn is that the mobilities of isotopic ions are well within 1 per cent of equality, and that those workers are substantially justified who contend that volume is the decisive factor in determining ionic mobilities. Later theoretical advances, it must be added, support this view, although the matter is still not absolutely settled (Jette, 1927).

Although all the work on isotopes, as has been stated, led to no successful separations, yet the positive results obtained in the test experiments on known mixtures did inspire the hope that the ionic migration method might, after all, prove of practical service in the separation of more familiar materials which are ordinarily obtained by the chemist in a pure state only with extreme difficulty. The most important example of this type is furnished by the family of the rare earth metals. The group of elements known as the rare earths comprises the elements of atomic number 57 to 71 inclusive, and yttrium with atomic number 39. These elements are distinguished by such extraordinary alikeness in chemical and most physical properties, due to the identical arrangements of the two outer shells of electrons in their atoms, that they actually approach isotopes in similarity. It is necessary, in practice, to take advantage of slight differences in solubility observed for corresponding salts and to resolve a given mixture into its components by a long series of fractional crystallizations or precipitations, ranging in number from several hundred to many thousand, according to the elements present and the degree of purity desired. These operations are so laborious and time-consuming that chemically pure samples of individual rare earth compounds are practically unknown, save on the shelves of a few skilled workers in the field of atomic weights. The 'rareness' of the rare earths is due not so much to the lack of abundance of their ores in Nature as to the lack of a simple method for their separation.

The first separation attempted was upon a mixture of yttrium and erbium, kindly furnished by Prof. James, of the University of New Hampshire. Potassium was used as a preceding faster ion and trivalent chromium as a following slower ion. Not only could the position of the rear boundary be more conveniently followed in this particular case because of the colour of the chromic solution, but it was also found possible to observe the actual progress of the separation within the rare earth section in a very simple way by means of a small direct vision hand spectroscope. The majority of the rare earths give solutions with characteristic absorption spectra, and, by noting the increase or decrease in intensity of the most prominent lines in various parts of a section, the experimenter could immediately detect any change in composition in the whole length of the section without interrupting the run. Here yttrium rapidly accumulated in the front half of the section and erbium was relegated to the rear. After a run of fourteen days, during which the boundaries moved two metres, an almost perfect separation had been effected.

The next mixture tested consisted of neodymium and praseodymium, two elements which are so nearly alike that their first differentiation by Otto von Welsbach still forms one of the triumphs of technique in this difficult field. Neodymium salts exhibit a purple solution in water, while praseodymium salts give a green solution. A beautiful crystal of neodymium nitrate and an equally fine specimen of praseodymium sulphate were secured from the Chandler Chemical Museum of Columbia University and the careful work of several years was deranged in a few minutes by mixing these two salts to obtain a solution with an intermediate neutral tint. With this solution as a central section and with the same arrangement as in the preceding experiment, it required only a few days migration to disclose the fact that the front portion of the section was becoming distinctly green and the rear portion purplish. Observation by means of the spectroscope simultaneously showed that the characteristic absorption lines of praseodymium were becoming more pronounced in the front section and fading out in the rear, with the opposite behaviour for the neodymium. At the end of ten days, substantial progress towards a final separation had been accomplished.

Experiments with other mixtures of rare earths showed that, while complete separation could not be secured in every case, yet in most mixtures the mobilities of the ions were sufficiently divergent to compel a very rapid concentration of one particular component in the front or in the rear section. The method may, therefore, be considered as a general new method for obtaining pure samples of the rare earth elements with the expenditure of much less time and trouble than is required by the classical method of fractional crystallization (see, however, Selwood and Hopkins, 1929).

Important industrial uses for the rare earth elements will undoubtedly be discovered as soon as

more convenient means for their isolation are developed. Aluminium remained a chemical curiosity until this same end was achieved, and while the later members of the aluminium family may not duplicate the successful career of their brilliant brother, yet it would indeed be strange if Nature had omitted to endow them, alone of all the elements, with no properties of service to mankind.

The success of the experiments with rare earths suggested that the method might be applicable to the separation of radium from barium. The concentration of radium from the barium residues of carnotite ores at present involves a very tedious series of fractional crystallizations, and, since it had already been found that barium could be separated from the other elements of the alkaline earth family by the method here under discussion, it appeared very probable that a similar separation from radium, the last member of this same family, could also be accomplished. Samples of barium residues containing known amounts of radium and of mesothorium (an isotope of radium) were obtained and, after a few days migration, the sections were cut into segments and their radioactivity examined. It was found that the radioactive components of each mixture tested accumulated very rapidly in the front part of the section. The ease of the separation in this case is so striking as to suggest that the ionic migration method may come into technical use for the concentration of radium in barium residues.

An attempt to separate hafnium and zirconium did not lead to such conclusive results. Hafnium exists to the extent of several per cent in all zirconium ores, and the similarity in properties is so pronounced that the actual discovery of hafnium was not definitely established until 1923. A sample of hafnium-rich zirconium oxide was kindly furnished us by Prof. Hevesy. The elements were not amenable to separation by the ionic migration method in the form of positive ions, owing to hydrolysis, and only after considerable search was a suitable negative complex ion discovered in the form of a complex oxalate. After long migration, analysis showed a very slight accumulation of hafnium in the rear of the section, the mobilities of the complex anions being evidently so close together as to render a complete separation impracticable.

The results in an entirely different field have been of better promise. Many of the alkaloids particularly useful for medicinal purposes are derived from natural sources as mixtures of several individual members which can be separated by the ordinary methods of organic chemistry only with great difficulty. The alkaloids, however, are weak bases which form soluble hydrochlorides, and the mobilities of the cations of these salts are not identical. It should, consequently, be possible to obtain a pure sample of an especially valuable alkaloid from the mixture of similar materials with which it naturally occurs by use of the ionic migration method, and several preliminary experiments indicate that the method is indeed applicable in a majority of cases.

Ionic Discharge Potential

Isotopic ions differ only in their minute nuclei, and these are surrounded by a relatively distant cloud of external electrons, increasing in number as we proceed up the periodic system. When an ion is discharged at one of the electrodes during the process of electrolysis, all that happens is that one or more electrons is added to, or taken from, this external cloud. Although, therefore, the free energy changes involved in the discharge of two isotopic ions are not necessarily identical, it is unlikely that the discharge potentials will be markedly different in the two cases, when expressed in volts. If we could pass an electric current through a solution containing two isotopic ions, adjusting the voltage so that the decomposition potential of one species was *just exceeded* while the decomposition potential of the other species was *just not attained*, then a perfect separation should be feasible. Owing, however, to the impossibility of adjusting the voltage with sufficient delicacy, and to the fact that reverse interaction of the discharged material with the solution, unless its immediate withdrawal from the system was effected, would probably tend to re-establish equilibrium conditions fairly rapidly, it appeared that the utmost that would be attainable in practice would be an 'electrolytic fractionation'.

Encouragement that a partial separation might be achieved by this method was derived from the knowledge that it is possible to vary the composition of a tin-lead alloy, obtained by electrolysis of a mixed solution of tin and lead salts, very significantly by varying the voltage employed. The discharge potentials of tin and lead ions differ by approximately 0·01 volts, so that it seemed that there was a likelihood of being able to establish some slight degree of separation of isotopic ions by fractional electrolysis, even if their discharge potentials were still more nearly identical.

The choice of an element to test the method presented great difficulty. Other things being equal, a light element would obviously be preferable, since the external electrons are fewer in number and shield the nucleus less effectively. The experimental obstacles in the case of lithium, the lightest known isotopic element at the time when this work was started (1921), are, however, exceptionally grave, owing to the activity of the free metal and to the elaborate technique necessary for its exact atomic weight determination. Other light elements presented similar objections, and the selection finally fell upon mercury. It is true that mercury is very near the wrong end of the periodic system, its isotopes ranging in atomic mass from 197 to 204, but the free metal is inert and the smallest change in its average atomic weight can be immediately and readily detected by means of density determinations. It was consequently decided to attempt the fractional electrolysis of mercury first, and to continue with lithium later, if the results obtained were sufficiently promising.

The results obtained have been presented in detail in a Ph.D. thesis from Columbia University (Haring, 1924). Electrolysis of an acid solution of mercurous nitrate, using a low voltage and low current density, gave mercury with a density only 0·999981 that of ordinary mercury. This value was the average of fourteen different electrolyses. Parallel experiments in which a high voltage and high current density were employed gave mercury with density unchanged.

The reduction in density, it will be seen, was only nineteen parts per million, which represented three times the average deviation of the individual results. The greatest difficulty encountered in the whole investigation was the exact setting of the mercury level in the pycnometers and, although the error involved thereby was minimized by repeating each

setting five times and averaging the results of the five weighings, the probable error involved still amounted to 7·5 parts per million. This fact, together with the disappointingly small change (0·004 unit) in the average atomic weight of the electrolysed mercury, discouraged us from proceeding further at the time.

I have always been convinced, however, of the reality of the fractionation indicated and, in the expectation that more conclusive results would be obtained with a lighter element than mercury, Mr. McLaren of Syracuse University began a systematic study of the electrolytic method of isotopic fractionation under the direction of myself and Dr. Ludlam at the University of Edinburgh in 1930. A considerable amount of work had been done on lithium, but no final results attained, when the discovery of the isotopic character of hydrogen turned our attention early in 1932 to that element, which obviously offered still greater chances of success. Spectrographic evidence suggesting that the proportion of the heavier hydrogen isotope in the first fraction of the electrolysis of water was less than in ordinary hydrogen had been obtained when parallel investigations in the United States were brought to a successful conclusion. The isolation of pure 'heavy' hydrogen and the preparation of pure 'heavy' water from the final fractions of the repeated electrolysis of ordinary water must be regarded as one of the most significant achievements in chemistry in recent years. It is disappointing to have been anticipated, but there is some consolation available in being the first to employ the method by which the separation was made.

Thermal Diffusion

Since 1933, striking advances in the separation of the isotopes of heavier elements have been made, notably by the chemical exchange method of Urey, but perhaps the most promising general line of attack is that opened up recently by the work of Clusius and Dickel on thermal diffusion.

This property was originally noted by Ludwig in liquids so long ago as 1856. Ludwig placed a solution of sodium sulphate in a U-tube, one limb of which was kept in boiling water and the other in melting ice, and found that after two days solid crystals had separated out in the cold limb, while the concentration in the hot limb had decreased to one half. Soret (1879) made measurements of the same kind with various salts, employing vertical tubes with the upper part heated and the lower part cooled, and his results were utilized, not very justifiably, by van 't Hoff (1887) in confirmation of the applicability of the gas laws to solutions.

Thermal diffusion in gases was first discussed by Enskog (1911) and predicted independently by Chapman (1917). In the latter year, Chapman and Dootson achieved a partial separation of a mixture of carbon dioxide and hydrogen contained in two glass bulbs connected by a capillary tube, with a temperature difference of 210°. The theory of the process, both for gases and for liquids, however, remained obscure, as will be evident from the fact that Chapman, in 1929, pointed out that existing theories did not even indicate with any certainty the sign of the slight amount of fractionation generally obtainable.

Only in 1938 did thermal diffusion suddenly come into practical prominence through the astoundingly simple discovery that the substitution of a horizontal for a vertical temperature gradient enhanced the effect to an enormous degree. In the older forms of apparatus, convection currents were deliberately avoided, but Clusius and Dickel ingeniously used such currents to assist the normal thermal diffusion process by what has been called 'thermo-syphon action'.

This action is illustrated in the accompanying sketch, where the convection currents set up in a narrow rectangular vessel with opposite heated and cooled surfaces are indicated by arrows. Normal thermal diffusion establishes a small concentration gradient in the direction AB, the concentration of the lighter component of the gas or solution contained in the tube increasing at A and decreasing at B. The upward convection current consequently causes the ascent of excess lighter molecules, the downward current the descent of excess heavier molecules. A second concentration gradient is thus developed in the direction CD, and the steady continuation of the process may ultimately establish, under suitable conditions, a complete separation of the two components of the original mixture.

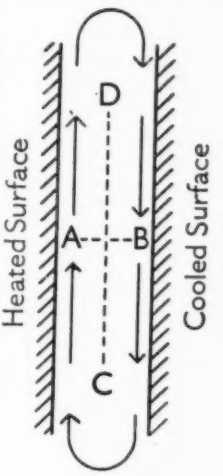

Fig. 2.

An improved form of apparatus devised by Clusius and Dickel for use with gases consists of a long vertical glass tube, externally cooled, with an electrically heated platinum or nichrome wire running down its centre. With a series of successive tubes, altogether 36 metres in length, Clusius and Dickel succeeded in obtaining from ordinary hydrogen chloride top samples corresponding with a 99·6 per cent content of the lighter isotope of chlorine and bottom samples corresponding with a 99·4 per cent content of the heavier isotope. This represents so far the only reasonably complete separation secured by the thermal diffusion method, but the applicability of the method to other cases is obvious.

Considerable work, though practically all of a preliminary nature, has also been done on thermal diffusion in liquids since 1938, and I shall conclude with an account of some simple experiments carried out in my own laboratory by Taylor and Ritchie. The solutions employed are enclosed in a vertical annular space, less than 0·5 mm. in diameter, between two concentric glass tubes, approximately 1·5 metres long. A temperature gradient of about 80° is maintained across this space by the passage of a current of cold water through the inner tube and a current of steam through a surrounding jacket. Eight hours of thermal diffusion of a solution of copper sulphate fractionates it so thoroughly that a goodly crop of crystals will separate out at the lower end, while the characteristic blue colour of the salt will entirely disappear from the uppermost section. Two hours of thermal diffusion of a solution of copper bromide suffices to develop the characteristic reddish-brown concentrated coloration and the bluish dilute colour at opposite ends of the tube. A solution of cobaltic chloride after two hours of thermal diffusion clearly exhibits the deep blue and pink of concentrated and dilute solutions at its upper and lower ends.

It will be evident that thermal diffusion represents a most valuable new tool at the disposal of the chemist, facilitating separations of a most diverse character. One practical point that has already come up for investigation is the influence of thermal diffusion on ingot segregation in steels. Metals are such good conductors of heat that it seems unlikely that any steep temperature gradient could persist for a sufficient length of time in the liquid contained in an ingot mould for thermal diffusion to induce significant segregation. At the suggestion of Dr. C. H. Desch, however, an experiment to test this point was undertaken in my department by Dr. Ritchie. The apparatus employed was similar to that already described, but the annular distance was reduced to 0·25 mm. and the length of the concentric tubes increased to 2 metres. The annular space was filled with a solution of 1·5 per cent zinc (density 7·1) in mercury (density 13·6). Even under such favourable conditions, no fractionation whatever was observable after a run lasting for 32 hours.

This negative result is no doubt largely due to the high thermal conductivity of the amalgam, which reduces the temperature gradient by making the drop between the annular liquid surfaces much less than that between the surrounding glass walls. The same factor, however, as has been noted above, should also diminish the effect of thermal diffusion in ingots. Further work in the field is obviously necessary, but in the meantime the view expressed by Desch that thermal diffusion cannot play an appreciable part in producing the observed segregation in steel ingots appears to be fully justified.

FUTURE OF THE POPULATION OF GREAT BRITAIN

By COLONEL SIR CHARLES ARDEN-CLOSE, K.B.E., C.B., C.M.G., F.R.S.

AN interesting memorandum, entitled "Current Trend of Population in Great Britain", was presented to Parliament by the Minister of Health and the Secretary of State for Scotland in May 1942*, but it is explained, in the opening paragraph, that this memorandum was actually "submitted shortly before the outbreak of the war by the Registrars-General of England and Wales and of Scotland to the Royal Commission on the Geographical Distribution of the Industrial Population". It was not included in the papers published by that Commission. The facts dealt with are those available up to April 1938 and no account is taken of the War.

The memorandum is in the form of a compact little pamphlet of twelve pages, with tables and two diagrams. The account is somewhat compressed and requires attentive study. It brings our knowledge of the facts reasonably well up to the date mentioned, and, with the aid of certain assumptions, gives indications of what our numbers might be twenty or thirty years hence; but it has to be borne in mind that the assumptions do not include the possibility of war.

The main intention of the memorandum is stated to be the presentation of the facts in as objective a manner as possible, so as to correct misunderstandings based on rather sensational forecasts as to what might be the fate of the population of Great Britain a century or so hence. A warning is given that forecasts extending to such a distant future have very slight value.

It is stated that, "as long ago as 1926, attention began to be drawn in official reports to the significance of the phase which the falling birth-rate was then entering". The crude birth-rate, which reached its maximum of 35·4 per thousand of the population in the period 1871–75, after that showed a continuous decline until the year 1933 when it reached the low figure of 14·7, "from which time the fall appears to have been arrested and a minute, though so far uninterrupted, reverse of the movement taken its place". The estimated figure given for 1938 was 15·5, but actually it was 15·1. We can bring the figures up to date, thus: 1939, 14·9; 1940, 14·6; 1941, 14·2. These figures are later than the date of preparation of the pamphlet; they are obviously affected by the War and will not serve as a guide to future peace-time rates, for it must be remembered that great wars, such as the War of 1914–18, or the present War, do materially diminish the populations of the countries engaged in them; the population of Great Britain was probably some two millions less at the time of the census of 1921 than it would have been had there been no war a few years before.

A table is given to show the average annual percentage of increase of our population, and from this it appears that between 1871 and 1911 the average annual increase was 1·12, but after 1911 there was a marked drop and the average increase up to 1938 was about 0·45. The most striking drop was from 0·98 in 1901–11 to 0·46 in 1911–21, that is, the most marked change occurred in the war period "when the rate of increase, which, prior to that time had been of the order of 1 per cent per annum, dropped to rather less than half that figure, at which level it has been maintained ever since".

It is well known that since 1923 the birth-rate has not been sufficient to provide a stationary population; in fact the births are only, by Dr. Kuczynski's calculation, about three quarters of the number required to reach that standard, in spite of the present continued increase of our numbers. Later information puts the net reproduction-rate, in 1938, as 0·84, so that fertility would have had to be raised by about one fifth to result in a stable population. To illustrate the "transitory and unbalanced nature of the present age distribution" a comparison is made between this and two theoretical stationary populations, resulting in each case from a constant annual number of births of 700,000, with different rates of mortality. It is stated that the comparisons show that the population would be maintained at or near its present level of forty-six millions if there were continued the flow of 700,000 births which were registered in 1937. But, of course, the supposition of a continued, constant flow of this kind "could only be realized by a gradual rise in fertility over the next two or three decades". It is added that such a recovery might possibly be secured "with the aid of measures ensuring a national population policy".

The male section of the important group 15–45 is now much smaller than the female section of that group. It is possible that this inequality is not sufficiently stressed by demographers. The marriage-rate must be adversely affected by such a disparity. We all of us know of women who have lost their prospective husbands in the War of 1914–18, and of others who found themselves in a world which

* H.M. Stationery Office, 1942. 2*d.* net.

contained too few marriageable men. If we think only of the number of marriageable women we shall be apt to forget the question of whom they are to marry. The Registrar-General for England and Wales reported in 1940 that there were more than a million and a half surplus women of marriageable age in the middle of 1939. The present War is not going to make this state of things any better.

An important consideration, too often neglected, is brought forward with regard to mortality. Too often it is assumed that the death-rates at the different ages have improved so much during the past two generations that further significant improvement is unlikely. Here we have clear indications to the contrary. A couple of diagrams are printed which show, in graphic form, the improvement in the death-rates of males and females, not only in the early stages of life, but up to, and including, the decade 75 to 85, in fact, practically for the whole of the normal life of man. In the construction of the diagrams the "death rates experienced in Great Britain over successive decennial periods between 1861 and 1931" are recorded for a series of age groups from 10 to 85. The general character of the curves so obtained is the same for all groups—all show an improvement. The curves have been extended by dotted, free-hand lines, which may be taken to show the probable death-rates at the various ages for the next thirty years. The ordinates (death-rates) are plotted on a logarithmic scale.

The authors are satisfied that this simple way of predicting the future is justified by consideration of statistical continuity, of the effect of curative medicine, and of the fact that some other countries have mortalities lower than Great Britain. It is noteworthy that the Registrars-General should have directed attention to the improvement in British mortality. There has been too much silence on this point on the part of unofficial persons and bodies who have, in recent years, studied the question of the future of our population. The Registrars-General remark that they "find no justification for the assumption, implied in some of the contemporary population forecasts . . . that the most probable course of mortality is one which shows no significant improvement on the rates actually experienced in recent years".

So much for mortality; and now for fertility. The line taken is that there was no diminution of the crude birth-rate in Great Britain after the low level (14·7) of 1933; that there was, in fact, a slight improvement. The values for the five years 1934–38 were 15·1, 15·1, 15·2, 15·2, 15·1. (The figure for 1938 was not actually available when the report was written, and an approximate value of 15·5 was given.) The pamphlet directs attention to the steadiness of the rate during these years, and it is remarked that "it would be unwise and unjustified to assume that any future departure from the present level is more likely to be in one direction than the other". It may here be noted that the Population Investigation Committee, in its publication "The Future of our Population", issued in 1936, put the average crude birth-rate in England and Wales as likely to be 12·4 for the five years 1935–39. This has proved to be wide of the mark.

As for the years 1939, 1940, 1941, the crude rates were 14·9, 14·6, 14·2; but these are war years and the only proper inference is that a world war has a disturbing effect on the birth-rate; this, indeed, is only common sense. The Registrars-General, then, make use of a constant fertility rate. The table on p. 9 of the pamphlet, which shows the effect of applying this rate, is somewhat difficult to follow, but we can accept the statement that the birth-rates were calculated with no bias in one direction or the other. Of course the calculations were made before the outbreak of the War and give no guidance for the war period, which may be a long one.

Since the pamphlet was published there has become available further information, based upon the data provided in accordance with the Population (Statistics) Act of 1938. From this it appears that married fertility has declined since 1921, at all ages of the mothers, decreasing to about two thirds of its previous value at 40–44; and the rate of child-bearing has decreased with the interval since marriage. This change is only explicable by deliberate family limitation.

The final factor dealt with is migration. It is assumed that, since the net outward migration from Great Britain for the years 1924–30 was 1·2 per thousand of the population, and the net inward migration for the years 1931–37 was 1·4 per thousand, that this inward migration would continue "up to 1941, followed by a gradual reduction to vanishing point in 1951, and nil thereafter". This forecast is now, owing to the War, valueless.

With these three factors, mortality, fertility and migration, thus determined, the authors proceed to gather up the results so far as concerns the numerical future of our population. They say that in their calculation there has been no conscious bias in either direction. There is, of course, a margin of uncertainty in the assumptions, and this uncertainty must render distant forecasts increasingly unreliable. But they make allowances for favourable and unfavourable variations in the basic assumptions, and give the effect of a ±10 per cent variation in the ultimate mortality and fertility to be reached in 1971, with proportionate differences in the intervening years. No variation is made with regard to migration. It is added that the forecasts "make no allowance whatever for such change as might ensue from the possible introduction of a controlled population policy in regard either to the birth rate or to migration".

Here are some of the final results:

TOTAL POPULATION OF GREAT BRITAIN (thousands)

Actual population 1937	1941	1951	1961	1971
46,008	46,565 ± 25	47,501 ± 280	47,192 ± 805	45,980 ± 1,579

Taking the lowest figure, we might, but for the War, have expected a population in 1971 of about 44½ millions, in place of the 1937 population of 46 millions. What is a matter of some concern is the age distribution which is shown in the same table. We see that in 1971, according to these calculations, the number of children under fifteen will have come down from 10 millions to 7½ millions; the numbers in the working ages 15–45 will have been reduced from 21½ millions to a little over 17 millions; and the category "over 65" will have increased from 3·9 millions to 7·9 millions. These are the troublesome features of a birth-rate which is insufficient, ultimately, to provide a stationary population. The mere reduction of total numbers might have certain advantages in such a very densely

populated country as England. But the overloading of the country with people past work, or, at any rate, not equal to exerting the energy expected of the middle-aged, has no advantage at all.

These calculations would give a date for the attainment of the maximum population some time between 1951 and 1961. An unofficial estimate made in 1927 gave the year of the maximum as between 1949 and 1953. Dr. Enid Charles's estimates, published in 1935, based upon three different assumptions, and very carefully worked out, gave the year of the maximum as 1940, 1944 or 1960. All that we can say is that the maximum has not yet (1942) been reached, and that all estimates have been upset by the War.

We cannot tell how long the War will last, or how many of our young and middle-aged men will be killed or permanently disabled. But we do know that there is the certainty of the War's disturbing effect on the birth-rate, which has already dropped from the recent pre-war average of 15·1 to 14·2 in 1941. Moreover, war takes its chief toll from the young men, and the result must be that the disparity between the numbers of males and females will be still further accentuated, as also will be the ageing of the population as a whole.

To take a long view then, it would appear that, while we need fear no serious diminution of our numbers for the next generation, the population will grow steadily older, and if we wish eventually to attain a happy condition of equilibrium, with a satisfactory age distribution, and thus avoid dangerous decreases of population in the times of our grandchildren and great-grandchildren, we should take steps as a nation, now, to encourage an improvement in the birth-rate.

THE HUMAN FACTOR IN PRODUCTION*

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THE word 'production', like its fellow 'output', is an uninspiring word. Those of us who must use it for convenience need to remind ourselves at intervals that it is a general term and really stands for concrete things. Behind each article is an innumerable company of human beings, each with his hopes and fears, dreams and realities. This may be a machine age; industry may be mechanized, but personally I have never met the mechanized human being.

The work of any human being, like any other activity, can be affected positively or negatively by numerous causes, and a complicated web has to be unwoven if these causes are to be sorted out. Nor are all of them equally operative all the time; now one, now the other is thrown into relief with the others in the background.

Output is affected by the material and the psychological environments and by the physical and mental make-up of the individual.

* Substance of a paper read on June 6 before a conference on "Social Biology in Relation to the War" arranged by the British Social Hygiene Council.

Factors in the Material Environment

(*a*) *Lighting.* Natural lighting (daylight) is ideal when there is plenty of it. Since it is not available for night work, and for the ordinary hours of work in winter, and since the black-out has interfered still further, a satisfactory substitute has to be found. With inadequate illumination, output is reduced by a slower rate of working and by an increase of spoilt work. The liability to experience eye strain, 'nervous trouble' (probably irritability) and accidents is increased.

(*b*) *Heating.* Lack of warmth on one hand, and overheating on the other, cause discomfort. Experience gained during the War of 1914–18, and by investigations made since, has shown that efficiency falls when the temperature rises unduly—particularly in heavy industries. In industries such as iron and steel, tin plate and glass manufacture, output is lowest in the hot summer months and highest in the winter. Even in the lighter work of textile operatives, the work deteriorates as the temperature rises above the comfort zone. On the other hand, low temperatures adversely affect manual dexterity. Broadly speaking, those atmospheric conditions which are generally found to be comfortable are also those which are best from the point of view of efficiency and safety.

(*c*) *Ventilation.* The first requirement is an adequate supply of chemically pure fresh air. After that, attention has to be paid to the temperature, humidity and rate of movement of the air. War-time lighting restrictions interfere considerably with ventilation arrangements. When a night shift is worked, arrangements for securing adequate ventilation are even more imperative. The problem of obtaining this adequate ventilation is not a purely mechanical one; there is also a psychological factor.

(*d*) *Hours.* Within limits, an increase of hours will increase the output. The human being is not a machine, and this being so, he can by force of will drive himself to perform far more than he considered his maximum. There were endless examples of this in industry after the Dunkirk period. To do this, however, reserve energy has been used; he has as it were used his reserve energy; and such activity can only be carried on for a limited time, after which considerable reduction occurs. The result is that after a continuous period of overwork, improvement in output does not take place for a considerable time after the return to normal hours. The Health of Munition Workers' Committee of the 1914–18 War summed up thus: "Misguided efforts to stimulate workers to feverish activity in the supposed interests of output are as useless as would be the cheers of partisans encouraging a long distance runner to a futile sprint early in the race."

Even with normal hours of work, the long spells of four and a half or five hours should be broken up, preferably by organized rest pauses and not by accidental interruptions due to waiting for materials, etc. Most people can face with equanimity work of about two hours' duration and can keep a steady pace, but the prospect of five hours' unbroken work often exerts a retarding influence on speed. The effect of any continuous activity, whether physical or mental, is associated with a gradually diminishing success in that activity. The stages at which full success is replaced by partial success and eventually by failure are not the same for everyone, or constant for the same person. Anything that reduces the

available energy of the worker, or his interest, will affect the stage at which reduction of effective work begins to take place.

Fatigue is therefore a relationship between the amount of available energy, the nature of the work, and the attitude of the worker towards it. The remedy for fatigue is rest, and if work has been within reasonable limits, a night's rest will restore energy. If, however, in addition to the effects of hard or prolonged work, rest has to be taken in an air-raid shelter, or rest is impossible on account of the competing roar of guns and bombers, energy is expended instead of being renewed.

Apart from the diminished accuracy and speed consequent upon a fatigued state, there are well-known fatigue sensations, namely, a feeling of weariness, disinclination to make an effort, inco-ordinated movements, organic disturbances, irritability, listlessness. Consciousness of these sensations can distract a worker's attention from the work to himself. Since some of these symptoms are associated with certain diseases, it is not difficult for a fatigued person to think he is ill; the step to being ill is short.

The Psychological Environment

(*a*) *The mental atmosphere.* Diminished efficiency, weariness and an appearance of listlessness *may* still be present when the work is light and hours normal. These pseudo-fatigue effects are due not to the using up of reserve energy but to lack of interest, and are associated with repetitive work, of which there is a great deal; the sufferer is bored.

It must not be assumed that all workers find all repetitive processes boring. One worker will stigmatize a process as boring and find another interesting, whereas to the observer there was nothing to choose between them. It has been found that on the average about 26 per cent of workers experience considerable boredom at repetitive work. An investigation was made into filament winding, soap wrapping, chocolate packing and tobacco weighing, from the point of view of the relation of output and boredom. Results showed that the more intelligent suffered more from boredom than those of inferior intelligence, but there were individual differences, even among the intelligent. It is worth noting that one can be fatigued without knowing it, but not bored.

There are compensations, however, even in repetitive processes, if we consider the worker as a human being and not merely as the performer of a repetition process. Some workers, for example, experience a sense of power when operating a large or delicate machine, and where a machine is a unit there is often a strong sense of possession: "my machine", it is called. Some people find considerable relief in day-dreaming and declare that it makes the time pass quite pleasantly.

Repetitive movements must either be studied as such, in which case they belong to the psychology of habit formation, or else put in their complete setting, including at least the opinions of fellow-workers and authorities about the work, physiological changes, emotional changes, and the general collective life of the place. The total reaction at any one time will be a reaction to a composite situation which constantly changes and of a personality that changes.

(*b*) *Personality of the Authority.* By far the most important factor in the mental environment is the personality of those in authority. There is no correlation between being able to do a job oneself, and being able to superintend others doing it. The effect of power on many people is to let loose a number of qualities otherwise held in check and usually unsuspected; in some occupations it has proved possible to demonstrate the effects of people in authority whose childish emotional development has counteracted their intellectual and practical ability.

There is a price to be paid for inadequate ruling and that price may be the unhappiness of subordinates. Unhappiness, however, does not remain merely a mental state; it receives expression, and in the industrial world this can sometimes be measured, the usual means of expression being an inferior output, high sickness rate or high labour turnover.

In one investigation, four groups of clerical workers were specially selected for experimental purposes to be comparable so far as possible; detailed output records for the six months preceding the experiments were available. Each group was in charge of a departmental head new to the group. Daily records were kept for the next two years. Hence it was possible to compare each person's record during the periods preceding and following the selection. The immediate effect of one head was to increase not only the average output of her group by 10·3 per cent, but also the output of each worker, whereas another decreased it by 5·5 per cent. The work was similar, and the chief difference between the two extremes lay in the temperament of the heads, one being a particularly 'vital' person, the other a person who 'enjoyed' ill-health.

Selection of People for the Right Work

A certain amount of negative selection takes place in most occupations; for example, a thin, weedy man would be unlikely to apply for a dock labourer's job, and a boy with little mathematical ability would scarcely seek to be an accountant. What is less obvious is the need for a much better selection for the majority, and the most important mental characteristic to test for is intelligence. It is now possible, within a very narrow margin of error, to test for general intelligence; it is also possible to differentiate between jobs needing intelligence and those needing little. To allow a person of great intelligence to do a job requiring little is wasteful, both to the individual and to the community; to use a person of lower intelligence than the job requires is to reduce output and cause unhappiness to the individual.

Since the War of 1914–18, when the United States produced the group intelligence test, much work has been done. In addition, a better adjustment of the worker to the work can be obtained by testing for various special skills and by systematic training. Experience in such varying trades as silk-weaving, chocolate covering, metal-polishing, folding and calender work in laundries, has shown that by systematizing movements and by training the novice a saving generally results. If a novice is allowed to pick up his job merely by watching proficients, he frequently adopts uncritically many unnecessary movements, which sometimes interfere with his possible speed and in some cases result in muscular habits harmful to him.

Those who are responsible for the teaching of beginners ought to have made a special study both of the technique of the movements and of teaching. Ability to perform an activity is no guarantee of an ability to teach it.

Time and motion study are a necessary part of the scientific study of skilled movement, and both are as necessary in industrial processes as in sport. If they are used merely to 'speed up' regardless of the individual, they are being used illegitimately and will have serious consequences. Any speed that results in a worker feeling flurried stands condemned. It is one of the anomalies of applied psychology that the earliest attempts to study skilled movement were made by two engineers, Taylor and Gilbreth, whose aim in studying the movements was to reduce the waste of human effort and by so doing to reduce the cost of production and enable the worker to earn more.

Suppose, however, that we know the requirements for a particular activity and that we have tests that are scientifically sound and practicable, and that the people in charge are adequate for the work of supervision. We have even then considered only part of the problem, for no account has been taken of temperamental or emotional differences, and the evidence shows that differences of this nature play an important part in many problems of industrial efficiency, sickness absence and labour wastage. In popular speech, 'temperamental' implies some lack of emotional stability. A healthy minded, well-poised person would scarcely be considered to have any temperament, whereas an emotional, erratic, irresponsible person is generally called 'temperamental'. In practical life, we hear more about temperamental failure than temperamental success. This is due to the nature of temperamental failure, which causes suffering, or at least mild discomfort, to those who come in contact with it.

Throughout history there runs a recognition of the importance of the general emotional make-up of individuals, for health, efficiency and happiness. Many observers, chiefly medical men and Fathers of the Church, that is, those whose work brought them into intimate responsible contact with human beings, have recorded as a fact of observation that there are considerable differences of an emotional nature between people, expressed very clearly in their susceptibility to, and behaviour in the face of, sickness and death, and also in their relation to other people, and as long as we have written records we have attempts at classification. The oldest and best-known comes to us from Galen in the second century, namely, the sanguine, the choleric, the phlegmatic and the melancholic. A more quantitative classification is: (1) those who are emotionally well balanced; (2) those whose temperamental organization is adequate for the duties required, but not altogether perfect; (3) those who are somewhat unbalanced, but within the limits of health, for example, persons rather too liable to be afraid or too easily stirred to anger but not thereby prevented from carrying on; their efficiency and happiness may be impaired but not seriously interfered with; (4) those whose temperamental lack of balance interferes with health.

The 'unbalanced' temperament is the so-called 'nervous' temperament. As a mode of behaviour it has been known for centuries, although not by that name. The name was given in the late eighteenth century and has stuck, though it has long since lost any meaning; the name refers to a mistaken belief that the symptoms characteristic of it were related to the organic 'nerves'. There are no tests for the 'nervous' temperament, and its assessment has to be made by a competent interviewer.

On the whole, after an examination of about a thousand people, there was some evidence showing that the nervous person tended to be less efficient in some environments than others, and also to be more difficult to get on with, to be less satisfied with his work and to have a somewhat higher sickness absence rate. Some environments, particularly those characterized by rigidity of organization and an arbitrary personal criterion of efficiency, are difficult for many nervous people.

The incidence of nervous symptoms varies considerably from one group to another. Those with serious maladjustment ranged from 1·2 per cent in a group of factory workers to 7 per cent in a group of clerical workers.

ABSORPTION AND EMISSION OF RADIATION IN THE ATMOSPHERE

THE radiation traversing the atmosphere is in part radiation coming from the sun, and in part radiation emitted by the earth and its atmosphere. The first is mainly in wave-lengths between 0·25 μ and 2·5 μ (1 $\mu = 10^{-4}$ cm. $= 10^{4}$ A.): the second is chiefly confined to wave-lengths greater than 5 μ, and may therefore be called long-wave radiation.

The passage of either kind of radiation through the atmosphere is impeded by Rayleigh scattering, by diffuse reflection from cloud particles, and by true absorption: we are concerned here only with the third of these. The major atmospheric gases, diatomic oxygen and nitrogen, do not contribute appreciably to the absorption, which is due to the presence of small quantities of certain polyatomic gases. Of these, the most important are water vapour (which is responsible for the major part of the absorption), carbon dioxide and ozone. Other gases, such as hydrocarbons, nitrogen oxides, cyanogen and sulphuretted hydrogen, may also contribute to the absorption; their total absorption is, however, small.

Ozone is largely confined to the upper atmosphere; if it were concentrated in a horizontal layer at atmospheric pressure at the ground, the thickness of this layer would be only 3 mm. Carbon dioxide, which is fairly uniformly distributed throughout the atmosphere, would form a similar layer about 2 m. thick. Water vapour is largely confined to the lowest few kilometres of the atmosphere. Its amount is very variable: if precipitated on the earth as liquid water, it would form a layer the thickness of which would range from less than 1 mm. on a cold dry day to a few centimetres on a warm, humid day.

Radiation from the sun is absorbed chiefly by ozone in the ultra-violet, between 0·2 μ and 0·3 μ, and in the visible region, between 0·45 μ and 0·65 μ; and by water vapour in the infra-red, especially between 0·8 μ and 2·2 μ. The ozone absorption results in a hot layer in the upper atmosphere, at a height of about 60 km. Apart from its effect in producing this layer, atmospheric absorption of solar radiation is relatively unimportant; with the sun at the zenith it reduces the intensity of the incident radiation only by 6–8 per cent.

Atmospheric absorption of long-wave radiation is much more important. A layer of damp air one metre thick can absorb one tenth of the radiation emitted from the surface of the earth. Successive layers of the same thickness do not increase the absorption proportionately; the atmosphere is fairly opaque in certain wave-lengths, but nearly transparent in

others. When the sky is clear, a quarter of the radiation leaving the earth may pass through the atmosphere without being absorbed. The atmosphere returns to the earth radiation of the wave-lengths which it absorbs. Because it is cooler than the earth, it returns rather less radiation than it absorbs; the radiation returned is, say, two thirds of that emitted by the earth. The quantities of radiation absorbed and returned vary very widely, however, with the atmospheric humidity.

The wave-lengths in which the atmosphere absorbs long-wave radiation can be determined by observations of the transmission of infra-red solar radiation through the atmosphere, such as have recently been made by A. Adel and his co-workers[1]. These show water-vapour absorption in a band between 5 μ and 8 μ, the amount of which varies with the humidity; it is nearly complete between, say, 5·6 μ and 7·2 μ. On one wing of this band, at 7·6 μ and 7·8 μ, there is absorption by nitrous oxide (N_2O) and possibly by nitrogen pentoxide (N_2O_5). Between 8 μ and 13 μ there is relative transparency, broken only by an ozone band between 9·1 μ and 10 μ; at its centre this band absorbs rather more than half the sun's radiation. There are weak absorption lines in the rest of the range between 8 μ and 13 μ, due mainly to carbon dioxide and water vapour, but they normally absorb less than 15 per cent of the sun's radiation in this range. A strong band of carbon dioxide absorption begins at about 13 μ, and cuts off the radiation completely by about 14 μ. This band should end at about 17 μ, but no radiation is actually observed beyond 14 μ. This is interpreted as showing that the rotational band of water vapour, which is known to produce large absorption at great wave-lengths, gives virtually complete absorption down as far as 17 μ. The argument is not altogether decisive, since the sun gives very little energy in these wave-lengths, and a small transmission might evade detection. Unfortunately, laboratory data are inadequate to decide whether the radiation beyond 17 μ is likely to be completely cut off by water vapour or not.

The incompleteness of laboratory data on the total absorption of gases in finite ranges of wave-length is a great handicap to workers in this field. Nearly all the experimental results were obtained before 1920. They do not agree with Beer's law, that the intensity of radiation is reduced in the ratio e^{-km} after passing through a column containing m gm. of absorbing material per sq. cm. of cross-section. Actually the total absorption A varies as a complicated function of m and of the pressure p. For air containing small quantities of water vapour or of carbon dioxide $A \propto (mp)^{\frac{1}{2}}$, roughly, for absorptions between 10 per cent and 50 per cent of the incident radiation; for very small absorptions, $A \propto m$; for large absorptions, A increases much more slowly than $m^{\frac{1}{2}}$. These results are explained by the line-structure of the absorption bands, assuming that the absorption coefficient k_ν due to any line varies with the frequency ν approximately according to Lorentz's collision-damping formula

$$k_\nu = \frac{i\alpha p/\pi}{(\nu - \nu_0)^2 + \alpha^2 p^2}.$$

If absorption is virtually complete at the line-centres, but line-wings do not appreciably overlap, this gives $A \propto (mp)^{\frac{1}{2}}$; if line-wings overlap, A increases more slowly than $(mp)^{\frac{1}{2}}$; if absorption is fairly small, even at the line-centres, we get $A \propto m$.

The line-width $2\alpha p$ is in general larger when due to collisions with like molecules than when due to collisions with unlike. Thus steam gives greater line-absorptions than air containing the same amount of water-vapour. Steam also gives wider absorption bands than damp air. Absorption at the wings of the bands is by molecules with large rotational energies, and the number of such molecules increases rapidly with increasing temperature. Thus, for example, in the range 11–25 μ, steam absorption is far greater than that of damp air, especially at the short-wave end of the range. This means that the difficulties inherent in the measurement of the absorption of long columns of damp air cannot be avoided by concentrating the water vapour into short columns of steam. Unfortunately, many of the experiments on water vapour absorption were made with steam.

The most complete set of measures with long atmospheric columns is that of Fowle[2], in which absorbing paths up to 200 m. in length were used. Fowle's measures were made with a rock-salt prism, and so are limited to wave-lengths less than 15 μ. Within this range his results agree roughly with those of Adel, quoted above, except, of course, that they did not reveal ozone absorption, which takes place in the upper atmosphere.

Beyond 15 μ, experimental data are scanty. To fill the gap, W. M. Elsasser[3] has recently attempted to calculate the absorption of water vapour from the observed positions and calculated intensities of the absorption lines, between 18 μ and 130 μ. Elsasser uses Lorentz's formula, quoted above, and determines the absorption A in the range in which $A \propto (mp)^{\frac{1}{2}}$. His estimates are subject to several causes of error, and one of his main conclusions, that line absorption is not large at wave-lengths below about 22 μ, is directly contradicted by the observations of Adel and others[4]. Nevertheless, his method is both interesting and suggestive.

Elsasser also predicted, on the basis of Lorentz's formula, that the wings of strong absorption lines near 50 μ would produce a continuous absorption in the region 10–15 μ. Adel obtained results agreeing with this prediction; but Strong[4] has recently set out evidence suggesting that what Adel observed was not a genuine continuous absorption, but a smoothed line absorption. There is no other evidence, experimental or theoretical, that Lorentz's formula is valid as far from the line-centre as Elsasser demands.

The above may serve to indicate how greatly workers on atmospheric radiation are handicapped by the inadequacy of laboratory data on absorption by water vapour. The absorption of carbon dioxide is more easily studied, since a mass of atmospheric carbon dioxide has nearly the same effect as an equal mass of the pure gas at the same pressure and temperature. Ozone absorption has not often been studied, and little is known about its variation with pressure and temperature—an unfortunate fact, in view of the great heights at which ozone absorbs.

Meteorologists have recently attempted to fill the gaps in the laboratory data by measurements on total atmospheric absorption and emission of radiation, summed over all wave-lengths[5,6]. A source of black radiation is viewed through an air column of variable length; the radiation received differs from that leaving the source because of absorption and emission in the air column. If the source is so cold that its radiation is negligible, the emission of the air is directly measurable; this known, the absorption of radiation from sources at other temperatures can be

found. The percentage absorption depends on the temperatures T_s, T_a of both source and air; it increases as T_a increases, but decreases as T_s increases, because of the increased importance of shorter wave-lengths, where water vapour absorption is smaller. The emission is equal to the absorption when $T_s = T_a$; considered as a fraction of the black-body emission at the same temperature, it decreases slowly as T_a increases. The results for long absorbing columns do not suggest that there can be any great transparency of water vapour beyond 17 μ.

Brooks[6] has recently indicated how such measures can be used as the basis for a study of atmospheric radiation. He has to ignore the variation of the percentage absorption and emission with T_s and T_a, and to assume that the quantities of water vapour, carbon dioxide and ozone in the atmosphere bear a constant ratio to each other. In theory, it is far better to consider the radiation in the separate wave-lengths; but because of the inadequacy of experimental data, Brooks's method has much to recommend it.

Another question, which has been studied for many years, is that of the total radiation reaching the earth from the atmosphere from all directions and in all wave-lengths. This is most easily studied at night, and is therefore usually called the nocturnal radiation of the atmosphere; it can, however, also be studied during the daytime. The radiation depends markedly on the ground temperature T and the vapour pressure e at the ground. Brunt has found that at any place it is given roughly by $\sigma T^4(a+b\sqrt{e})$, where σT^4 is the black-body emission at temperature T, and a, b are constants the values of which differ, however, from place to place. Brunt suggests that the variation in a and b may be due to differences in the instruments used by different observers; but part may well be due to differences of pressure of the temperature- and humidity-gradients, and of ozone emission.

Strong[4] has interpreted the factor $a + b\sqrt{e}$ in Brunt's expression as follows. At certain wave-lengths, air radiation is due to the emission of carbon dioxide and ozone, and does not vary with e; at others, water vapour is so opaque that it emits like a black body of temperature T, whatever e. Such wave-lengths contribute the term a. At yet other wave-lengths, water-vapour absorption is small and obeys the square-root law (in our earlier notation, $A \propto (mp)^{\frac{1}{2}}$). These contribute the term $b\sqrt{e}$. The explanation is suggestive, but incomplete, since there are wave-lengths in which water vapour is not opaque, but its absorption is too large to obey the square-root law, and these wave-lengths vary somewhat with the zenith distance and the humidity. The factor σT^4 in Brunt's expression also needs explanation, both since the temperature of the emitting air may differ somewhat from T, and because the emission in the wave-lengths at which the atmosphere is opaque need not follow the T^4 law characteristic of a black body emitting all wave-lengths.

Besides depending on T and e, the nocturnal radiation of the atmosphere must vary with the ozone content of the upper atmosphere. The ozone band between 9·1 μ and 10 μ comes in a range of wave-lengths where the atmosphere is otherwise nearly transparent, and so emission in this band at high levels appreciably affects the radiation reaching the ground. Conversely, radiation leaving the ground is able directly to influence the temperature of the stratosphere because of ozone absorption. Since it receives radiation direct from the ground, ozone tends to produce a higher stratospheric temperature than water vapour and carbon dioxide, which receive radiation from the cold upper troposphere. The effect of the ozone absorption is the more considerable because there is little water vapour at great heights. For this reason, Dobson has suggested recently[7] that stratospheric temperatures may be largely influenced by ozone absorption. His suggestion makes the temperature of the stratosphere depend on the relative amounts of ozone and water vapour present, and explains qualitatively the high stratospheric temperature at the poles, and the advance in phase of the annual variation of temperature in the stratosphere relative to that at any lower level.

[1] Adel, A., *et al.*, *Phys. Rev.*, **47**, 580 (1935); **49**, 288 (1936); also a series of papers in *Astrophys. J.*, beginning in vols. 84 and 87.
[2] Fowle, F. E., *Smithson. Misc. Coll.*, **68**, No. 8 (1917).
[3] Elsasser, W. M., *Astrophys. J.*, **87**, 496 (1938); *Phys. Rev.*, **53**, 768 (1938).
[4] Strong, J., *J. Franklin Inst.*, **232**, 1 (1941).
[5] Falckenberg, G., *Met. Z.*, **53**, 172 (1936); **55**, 174 (1938); and **56**, 72 (1939). Elsasser, W. M., *Mon. Weath. Rev.*, **69**, 1 (1941).
[6] Brooks, F. A., *Mass. Inst. Tech. Papers in Phys. Oceanography and Meteorology*, **8**, No. 2 (1941).
[7] Dobson, G. M. B., at a joint meeting of the Royal Meteorological Society and the Physical Society, held on May 20.

OBITUARIES

Dr. C. D. Sherborn

THE sudden death of Charles Davies Sherborn on June 22, within a week of his eighty-first birthday, came as a shock to his many friends. The staff and other workers at the British Museum (Natural History), with which he was continually associated since 1886, will mourn the loss of a distinguished and much-loved colleague, and will sadly miss the inspiration of an outstanding personality.

Born in Chelsea, he was the eldest son of Charles William Sherborn, the celebrated line-engraver and etcher. He received some of his early schooling at the hands of Miss Elizabeth Rye, then a noted educationist, and afterwards went to St. Mark's College School, Chelsea. At the age of fifteen he went into business, first with a bookseller and stationer, and later with a tailor. Then abandoning business for a scientific career, he set off for Switzerland and Germany, and during 1886–87 studied at the University of Strasburg.

At this period of his life Sherborn seems to have been first attracted to the study of geology and palæontology, and worked with Thomas Rupert Jones on the Foraminifera and Ostracoda. His "Bibliography of the Foraminifera" (1888), and his "Index to the Genera and Species of Foraminifera" (1896), indicate the first approaches to what was to become the ruling passion of his life. In 1890, in collaboration with Sir Arthur Smith Woodward, he published "A Catalogue of British Fossil Vertebrata", and at about the same time he catalogued the collections and library of the Geological Society of London, from which body he received the Lyell Fund in 1890. From 1892 until 1899 he was a member of the editorial board of *Natural Science*, and during this period prepared a new edition of a catalogue of the library of the Linnean Society of London. He was invited to examine and arrange the letters and papers of Sir Richard Owen at Sheen Lodge, and assisted in the preparation of the scientific part of Richard Owen's life of his grandfather (1894). Between 1894 and 1908 he undertook a survey of the "White Chalk

of the English Coast", in conjunction with the late Dr. A. W. Rowe, and surveyed western Cornwall with Upfield Green. The Geologists' Association, of which he was honorary secretary for nine years, published the results of the chalk survey.

Inspired by the projected "Index Kewensis" for plants, Sherborn put forward in 1890 a scheme for an "Index Animalium", designed to provide a complete list of all generic and trivial names from 1758 to 1850, with a reference to the date and place of publication of each name. This stupendous task, involving the extraction of more than 415,000 references from the original sources, proceeded steadily, and, in spite of a serious breakdown in Sherborn's health during the early stages, the first volume (1758–1800) appeared in 1902. The ten volumes comprising the second section (1800–1850) were published under the auspices of the British Museum (Natural History), and the final part did not appear until 1933—forty-three years after the inception of his self-imposed task!

Sherborn's published papers, many of them of a bibliographical nature, number nearly two hundred, commencing with one on Jurassic fossils in 1886 and ending with his extremely useful booklet "Where is the —— Collection?" in 1940. In addition, his bibliographical work led him to amass copious notes in manuscript, which have been consulted at the Museum by numerous workers and proved invaluable when a catalogue of the Museum's library was prepared by the late B. B. Woodward. He found time to investigate the history of his own family, a labour of love which resulted in "A History of the Family of Sherborn" (1901) and "A Sketch of the Life of Charles William Sherborn" (1912).

Sherborn was an honorary fellow of the Zoological Society of London, and served on the Publication and Finance Committees for many years; an associate of the Linnean Society of London; president of the Society for the Bibliography of Natural History; and an honorary life fellow of the Metropolitan Museum of New York. In 1931 the University of Oxford conferred upon him the honorary degree of doctor of science—a fitting recognition of his life's work.

Such, in brief outline, was the professional career of a man whose liberal culture and diversity of interest and activity were a constant surprise even to those who knew him well. Possessed of a very modest income, and living what to others appeared a most spartan life, he was able to indulge his passion for collecting stamps, coins, books, manuscripts, prints, and antiquities of all kinds. His valuable series of Byzantine coins, on which he was a recognized authority, was presented shortly before his death to the Fitzwilliam Museum, Cambridge, and his prints to the Art Department of University College, London. His many donations to both branches of the British Museum and to other institutions, together with his generous gifts to colleagues and friends, provided a practical expression of one of his most cherished convictions—that objects of special interest or value should ultimately rest in the hands of those who would best appreciate them.

Of the many qualities that endeared him to all, his kindliness, toleration, ready wit and equable temper, and, above all, his abundant generosity, stand out, and few who were admitted to friendship with "The Squire" can have failed to benefit by the association. His ready hospitality to foreign visitors and to other lonely folk provides but one example of his thought for others, and an invitation to "smoke and chat" at his house gave promise of an entertaining and instructive evening. His alleged dislike of women was probably a pose, and those wives of his friends who were lucky enough to gain his friendship soon discovered the kindly and sensitive nature beneath the cloak of gruff misogynism. He was unmarried, and is survived by two brothers.

J. R. NORMAN.

Mr. R. G. McConnell

RICHARD GEORGE MCCONNELL died in Ottawa, Canada, on April 1, 1942, in his eighty-sixth year. His death removed one of the last of Canada's outstanding geological explorers. He was born in Chatham, Quebec, on March 26, 1857, received his B.A. degree from McGill University in 1879 with first-class honours in natural sciences and in 1880 began geological exploration for the Geological Survey of Canada. His first work with the Geological Survey was done in Quebec province, but during 1881–1914 he was engaged every summer in geological studies mainly of reconnaissance and exploratory nature in the west and north-west of Canada. In 1914, he was appointed Deputy Minister of Mines, which position he held until his retirement from the Department in 1921. For several years thereafter he carried on consulting geological work and spent two summers on geological work for the Ontario Department of Mines.

McConnell's field work in the west as an officer of the Geological Survey covered a tremendous area. For five years he dealt with southern Alberta and Saskatchewan, providing much new information on the stratigraphy and coal areas. For three years he studied and mapped the Rocky Mountains west of Calgary, and was the first to recognize and interpret the great thrust faults along the mountain front. For four years he carried on geological mapping in the highly metalliferous West Kootenay area in British Columbia. For a ten-year period his work lay in Yukon, where among many important contributions may be mentioned his study of the Whitehorse copper belt, the Klondike placer field and his persistent recommendations for dredging which were later carried out with considerable profit. For eight years he worked in various parts of British Columbia, dealing mainly with the Portland Canal district, Texada Island, Finlay and Omenica Rivers and Rainy Hollow. For two years he covered ground north of Lesser Slave Lake, including the Athabaska tar sand area. For one winter he remained in the far north, and during the preceding and following field seasons made one of the longest and most remarkable Canadian exploratory trips when he travelled from Wrangell via Telegraph Creek, Dease River and Liard River to the delta of the Mackenzie, returning by way of Porcupine and Yukon Rivers to Skagway. As a result of this trip down through the Grand Canyon of the Liard he was the first explorer to recognize the fact that the Rocky Mountain system ended at latitude 60° N. The results of his field work were published on many geological maps and in some forty reports of the Geological Survey.

McConnell was a fellow of the Royal Society of Canada; he was elected in 1913 and president of Section 4 in 1918. He was elected a fellow of the Geological Society of America in 1889. He became a member of the Canadian Institute of Mining and Metallurgy

in 1898. He was also an honorary member of the National Geographic Society.

In November 1898 he married Jean T. Botterell of Montreal. His wife predeceased him by three years. He is survived by a son, Richard, on active service in Africa, and a daughter, Winifred.

He did not seek reward or high position; his ambition was to do the appointed task as well as possible and to further the science of geology. In order to be prepared for any work that he might be called upon to do, he kept himself in excellent physical condition by taking long walks. He took hardships as a matter of course, but nevertheless he could tell interesting stories about them afterwards when surrounded by personal friends, though he invariably declined to tell of his work or experiences on a public platform. After his return from an expedition he would immediately write his account of it in excellent English, and for the rest of the winter or spring would study reports of other geologists to keep up to date in his chosen field. He was fond of the outdoor life, and his keen powers of observation made him an outstanding field geologist. He loved the hills and was reluctant to give up his summer explorations to take on office duties as Deputy Minister. His body was cremated in Montreal and the ashes were scattered over the country he loved so much.

GEORGE HANSON.

Major G. W. Dunkin

MAJOR G. W. DUNKIN, lately director of the Agricultural Research Council Field Station, Compton, Berks, died on March 21 of this year. After post-graduate study, taking the diploma of veterinary hygiene of the University of Liverpool, he settled in general veterinary practice in Kent. Three or four years later, on the outbreak of war in 1914, he joined the Royal Army Veterinary Corps and spent most of his war service in Egypt, latterly commanding the 20th Veterinary Hospital in that country. He returned to practice after the War, but in 1923 was appointed superintendent of the Farm Laboratories Medical Research Council, Mill Hill.

From then on he carried out much important research on animal disease problems, and in this sphere his keen and inquiring mind and his early training and experience enabled him to plan and execute intricate large-scale experiments with domesticated animals and bring these to successful conclusions, avoiding much of the incidental, often unforeseen, complication that can delay and spoil such work on occasion.

Undoubtedly Dunkin's name will be remembered in future more particularly in connexion with the successful investigation into the causation of distemper in dogs carried out in collaboration with the late Sir Patrick Laidlaw. This is a classic in epidemic disease research. Among diseases to which Dunkin devoted much time are Johne's disease of cattle and bovine contagious abortion. He spent years on the study of Johne's disease, particularly with a view to the elaboration of a reliable diagnostic agent and to the production of a vaccine. He obtained important results, and though the problems have not been solved his contributions will be remembered.

In 1937 Dunkin was appointed to the Compton post and the intervening years were devoted to the organization of the estate to suit the requirements of the Agricultural Research Council, and to the inauguration of a large-scale research scheme on bovine contagious abortion.

Major Dunkin was a very active man in many spheres. He had held high office in the National Veterinary Medical Association of Great Britain and Ireland and on the Council of the Royal College of Veterinary Surgeons. He was chairman of the examination committee of that body. He was chairman for some years of the editorial committee of the National Veterinary Medical Association and chairman of a special committee of the Association to investigate small animal euthanasia, an important report being published under his guidance. He held the office of president, Section of Comparative Medicine, Royal Society of Medicine.

NEWS and VIEWS

Sir Henry Tizard, K.C.B., F.R.S.

BY electing Sir Henry Tizard as their president, the Fellows of Magdalen College, Oxford, have made a timely and important break with Oxford tradition, for he is the first man of science to become a head of a college there. When the country can spare him from his present extremely important duties as a member of the Air Council and of the Advisory Council of the Ministry of Aircraft Production, he will return to his old college with a mind enriched by an exceptionally wide and varied experience in the arts of war and peace. He won his spurs in scientific research under the guidance of Prof. Townsend, and his valuable work with Dr. Pye on adiabatic compression of gases made a link with internal combustion engineering which he has maintained ever since. The War of 1914–18 turned his thoughts and efforts to aeronautical research, and he has long exerted a highly important directing influence on British aircraft development, and on the solution of the problems of air warfare, offensive and defensive; these services have stood the nation in good stead during the present great struggle for a free world. The guidance of State assistance to research in pure science and in industry occupied him for eight years between the two Wars, and from that he turned to the administration of scientific and technical education, as rector of the Imperial College of Science and Technology, which has greatly prospered under his leadership. At Oxford, as in London, it is certain that he will not only be successful as head of his College, but also take a prominent part in science, education and national affairs.

Dr. W. S. Gordon

IN the obituary notices in this issue reference is made to the work of the late Major G. W. Dunkin, who was director for the past five years of the Compton Field Station of the Agricultural Research Council. Dr. W. S. Gordon, senior bacteriologist, Animal Diseases Research Association, Moredun Institute, Gilmerton, Edinburgh, has been appointed to succeed Major Dunkin. A graduate of the Glasgow Veterinary College in 1923, Dr. Gordon was on the staff of the Wellcome Physiological Research Laboratories, Beckenham, for some years

and took up the Scottish appointment in 1930. At Beckenham he was associated with Prof. Dalling, and other workers in those laboratories, in the production of biological products, and particularly in investigations of anaerobic infections in sheep. In Scotland he has led a team of workers in investigating and dealing with certain sheep diseases, notably louping-ill, tick-borne fever, braxy and lamb dysentery.

At the time of Gordon's appointment, the causal agent of louping-ill had just been discovered at the Edinburgh institute. Dr. Gordon followed up this work, making a close study of the disease and of the filtrable virus responsible for it, and developed methods for the immunization of sheep against the infection. An early discovery in this later work was that two entirely different virus infections were associated with louping-ill, that due to the true louping-ill virus and another, hitherto not recognized, that was named the virus of tick-borne fever, the latter infection being in itself a less severe disease. Among other disease problems on which he has worked in Scotland is grass sickness in horses, and although the cause of the disease is still unknown he has made notable contributions to the literature on the subject. Dr. Gordon has been active in a wide range of veterinary activities in Scotland and has a close personal interest in, and a practical experience of, general farming.

Dr. W. E. Gaunt

DR. W. E. GAUNT has been appointed research director of the Ashe group of associated companies. Dr. Gaunt graduated from the Colour Chemistry Department at the University of Leeds and obtained his Ph.D. for work on biochemistry and physiology undertaken at the Leeds School of Medicine. Afterwards Dr. Gaunt joined Sir John Orr's staff at the Rowett Research Institute to work on nutrition problems, and just before the War went to Guy's Hospital Medical School as dental research fellow to do work of a similar nature. Since the War started Dr. Gaunt has gained further experience in organic chemistry at Guy's, working with Prof. C. S. Gibson, and has also increased his all-round knowledge of nutrition during a short spell with Imperial Chemical Industries Ltd. Dr. Gaunt's experience in several research fields will enable him to serve Messrs. Ashe Laboratories Ltd. in maintaining and extending on sound nutrition lines their numerous food products. We understand that Dr. Gaunt will be encouraged to continue work on fundamental problems, particularly on human protein requirements.

Newton as an Astrophysicist

A PAPER by Elizabeth Connor entitled "Sir Isaac Newton, the Pioneer of Astrophysics" appears as Leaflet No. 158 of the Astronomical Society of the Pacific (April 1942), which supplies a brief outline of the life and work of Newton. Up to the time of entering the University of Cambridge, Newton had not exhibited any extraordinary talent, and though his undergraduate days were profitable, those which followed immediately after he graduated were the most productive which any scientific worker ever experienced. The outbreak of the plague forced him to leave Cambridge, and practically all his time between August 1665 and March 1667 was spent at Woolsthorpe, where his exile allowed him to concentrate on some of his greatest achievements. Among these may be noticed his method of fluxions, the law of the composition of light, and the law of universal gravitation. During this period he gave attention to experiments with the refracting telescope, but because he believed that chromatic and spherical aberration could not be overcome, he turned his attention to the reflecting type.

Towards the end of 1668 Newton completed his first telescope with an aperture of a little more than an inch and a tube 6 in. long. His second telescope was exhibited at the Royal Society in 1672 and is still in the possession of the Society. Its principle was used as the model and forerunner of a succession of giant telescopes which have been constructed since the days of Newton and are still the astrophysicist's best tool. His "Principia" did not appear until 1687 and his real scientific career ended with its completion. Most of his later years were spent in London as Master of the Mint, and he had many honours bestowed on him, but there was no return of the originality of his earlier days. Nevertheless, he gave a considerable amount of time to working over the material in hand. In 1704 his "Optics" appeared and in 1713 and 1726 the second and third editions of the "Principia". Specialists in various branches of science still voice the debt of their sciences to Newton.

Benedict Dybowski

AMONG the Poles whose scientific work was conducted in and for Russia was Benedict Dybowski, a sketch of whose life is given by Dr. J. Borucki in the first issue of *Polish Science and Learning* (June, 1942: Oxford University Press. 2*s*. 6d.). Dybowski was born at Minsk in 1833. As a schoolboy he kept various animals and was apparently the first to observe the metamorphosis of Petromyzon. He studied medicine and biology at Dorpat (1856), Breslau (1857) and Berlin (1860), where he published minor researches, for example, on artificial insemination of bees. At Dorpat again, in 1861, he worked on a monograph of the fishes of the Baltic lands. However, he was arrested for his political beliefs, and though the sentence of deportation was at first revoked, he was sent to Siberia after the 1863 insurrection. Here he began by exploring the forest and steppe around Czyta. Later, at Kultuk, he made long journeys to the Saya Mountains and to Chamar, developing a vast philanthropic medical service among the native population, so that the governor, J. G. Skolkow, invited him to join an expedition to the tributaries of the Amur and Ussuri Rivers, penetrating as far as the Pacific. They had scanty means, having to make their own boats. Nevertheless, Dybowski sent extensive collections to European museums and developed a view that each region showed a special structure of animal life in close correspondence with the environment. He discovered a number of hitherto unrecorded species, including a deer on the Ussuri, and noted the differences between Siberian and Bengal tigers. The birds he collected provided the basis for Taczanowski's critical "Review of Siberian Ornithological Fauna".

With Godlewski, Dybowski spent several years studying the fauna of Lake Baikal, and his researches included biological, chemical, bathymetric and temperature observations. The molluscs and sponges collected at this time were later described in a classical monograph by Benedict's brother, Wladislaw. Dybowski's extensive researches had enabled his

friends to secure his release in 1876, but he continued his medical, scientific and humanitarian work in east Siberia. Here he bought horses and reindeer for starving natives and introduced rabbit and goat-breeding into Kamchatka to give the native population a surer livelihood. The last years of his life brought him many honours from his native Poland, for he lived until 1931, dying in his ninety-ninth year. His earnest wish for good relations between the Slav peoples was not realized in his life-time, but his work in this connexion may inspire others to work for the same ideal now that there is close collaboration between Poland, the U.S.S.R. and Czechoslovakia in the many common aims that link them with other lands in the struggle for liberty.

Infant Protection in Mexico

IN an article on this subject in the May issue of the *Boletín de la Oficina Sanitaria Panamericana*, Dr. Salvador Zubirán, Under-Secretary of Public Assistance, states that Mexico is trying to reduce its high infant mortality, which was 30·5 per 1,000 births in 1905 and 122·7 in 1939, by the establishment of child welfare centres which were opened in 1929. In 1937 they were placed under a single Government department, and child welfare became a social as well as a medical obligation of the Government, and lost its 'charity' implications. Important objectives include the integration of the home, establishment of foster homes for deserted children, adoption of orphans, pre-natal training for the mother, vocational training for her if she has to work, and aid from pregnancy until the child is six years old. A children's hospital is being opened in Mexico this year to serve as a centre for medico-social education, scientific investigation and the spread of pædiatric information.

Anthropology in Switzerland

THE *Bulletin der Schweizerischen Gesellschaft für Anthropologie und Ethnologie*, 1941–42, contains a list of members of the Society, a report of meetings held during 1941, and three original articles. Dr. Hans Dietschy advances a number of theories to account for and to explain the attributes and nature of the Aztec pantheon. Dr. Lucia Graf describes some late medieval skeletal remains which were excavated from Holderbank in the Solothurn Canton in 1940, and Prof. Otto Schlaginhaufen provides a very complete anatomical and anthropometric description of four skeletons from Darvela near Truns. Late Iron Age (la Tène period) material was found with these bones. The cranial indexes of the skulls were respectively 76·1 and 74·5 for two male specimens, and 79·4 and 82·8 for two female skulls.

Committee on Synthetic Rubber

SIR ANDREW DUNCAN announced in the House of Commons on July 22 the formation of a Committee to consider the question of synthetic rubber to be constituted as follows: Mr. F. W. Bain, chairman of the Chemical Control Board of the Ministry of Supply (chairman); Sir Edward V. Appleton, secretary of the Department of Scientific and Industrial Research; Dr. J. W. Armit, director-general of explosives at the Ministry of Supply; Sir Robert Robinson, Waynflete professor of chemistry in the University of Oxford; and Dr. F. Roffey, controller of chemical research at the Ministry of Supply.

Earthquake Registered at Kew

ON July 12, a strong earthquake was registered on the seismographs at Kew Observatory. It began recording with *iP* compressional on all three components at 05h. 17m. 47s. U.T. From a tentative interpretation, the *eS* wave registered on all three components (north-south, east-west and vertical) at 05h. 28m. 09s. U.T., which gives the epicentral distance according to the tables in use at Kew as 9,230 km. A full suite of pulses was registered, the maximum ground movement at Kew being 29 μ amplitude at 05h. 53m. 15s. U.T. on the vertical component. The earthquake finished recording at 09h. 05m. U.T.

Announcements

LORD LOUIS MOUNTBATTEN, who is a vice-president of the Institution of Radio Engineers, has given a prize to the Institution, to be known as the Mountbatten Medal; it will be "awarded to the candidate who has proved himself the best candidate amongst those of the Royal Navy or Air Force who have presented themselves for the Graduateship Examinations of the Institution held during the year".

THE British Museum (Natural History), Cromwell Road, South Kensington, London, will re-open to the public on August 1. Owing to shortage of staff and the evacuation of many exhibits, it is possible to open at present only very few of the galleries. The times of opening will be 10–4 on weekdays and 2–4 on Sundays. Small children must be accompanied by an adult or a responsible older child.

THE trustees of the Leverhulme Research Fellowships have approved the following awards for research, tenable for varying periods up to two years: Dr. M. R. Anand, for work on landmarks in Urdu literature; Dr. R. S. Bagnall, for a monograph of the British Apterygota, including springtails, bristletails, and other primitive groups; Miss A. M. Cameron, warden, Lady Margaret Hall Settlement, Lambeth, for the study of voluntary social service by wage earners.

WRITING with reference to the review entitled "A Fateful Legacy" in NATURE of July 18, p. 72, Prof. Macgregor Skene points out that the complete quotation from Darwin's "Origin of Species", and indeed the last sentence of the book, is: "There is grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being evolved."

REFERRING to the obituary of Sir Daniel Hall in NATURE of July 25, Mr. G. M. Johnson, head master, states that the school in which Sir Daniel Hall was so long interested, and in which he did such good work as chairman of the governors, is The Lord Wandsworth College, Long Sutton, Basingstoke, Hants, and not as stated in the obituary notice, at Wantage. Sir Daniel Hall was taking an active part in the life of the College until he entered the nursing home in London, some three weeks before his death.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications.

Distribution of Attack on Iron or Zinc Partly Immersed in Chloride Solutions

IRON or zinc partially immersed in sodium or potassium chloride at first remains unattacked at the water-line. Corrosion occurs at a zone somewhat below the water-line, and researches at Cambridge[1] have indicated it to be connected with the electric current flowing between this zone as anode and a cathodic zone at the water-line, where oxygen, the cathodic stimulator, can most easily be renewed. The corrosion rate has been found to be approximately equivalent, in the sense of Faraday's Law, to this current, which, for solutions of high conductivity, may approach the limiting value given by the intersection of the anodic and cathodic polarization curves; for less conducting liquids, it is that value i, which will make an intercept iR, where R is the resistance. Thus the short-period phenomena have been fairly well cleared up. However, the long-period corrosion researches of Bengough and Wormwell[2] at Teddington have shown that finally both metals became more attacked at the water-line than anywhere else. The violent attack at the zone previously immune tended to suggest that the mechanism of corrosion in the later stages was different from that in the early stages; but by introducing the essential distinction between probability and conditional velocity[3], a common explanation appears for the seemingly divergent facts.

If the probability that corrosion will commence at a given point within a time element dt be defined as $p\,dt$, the chance that the point will escape corrosion for time t and then commence to corrode in dt will be $e^{-pt}\,p\,dt$. The 'expected' corrosion over an experiment lasting time T will be

$$\int_0^T e^{-pt}\,pu\,(T-t)\,dt, \quad \text{or} \quad uT - \frac{u}{p}\,(1 - e^{-pT}),$$

where u is the conditional velocity (mass per unit time and area). This approaches the limiting values of $\frac{1}{2}upT^2$ and uT, as T approaches zero and infinity respectively.

Now p will be minimal at the water-line, since the cathodic product (sodium or potassium hydroxide) is a powerful inhibitor; on the other hand, u will be maximal at the water-line, although it will vary little with level, unless the 'intercept-correction', referred to above, is considerable. Numerous measurements of potential at different distances from the water-line have indicated[4] that the intercept-correction is small, except in very dilute solution; we may expect, therefore, that u will be maximal at the water-line, but that its variation with level will be much less than that of p. Consequently, in the early stages where u and p enter symmetrically into the expression $\frac{1}{2}upT^2$, the expected corrosion will in effect be least where p is lowest, thus explaining the immunity at the water-line. In the later stages, where p does not enter into the expression (uT), the expected corrosion will be greatest at the water-line, where u is highest. Thus we arrive at an explanation of the apparently discordant facts that *the water-line is unattacked in the early stages and severely attacked in the later stages.*

Other possible causes for the severe water-line attack in the advanced stages may deserve consideration, but for the moment the mechanism suggested seems capable of explaining the known facts. The argument just sketched has, however, neglected the possibility that the value of p or u may be shifted by previous occurrences at the same point, or by simultaneous occurrences at neighbouring points. The accumulation of alkali at the water-line (if not dissipated by stirring or by conversion to carbonate) may cause p to fall with the time, thus postponing, perhaps indefinitely, the breakdown at the water-line. Experiments by Dr. T. P. Hoar and by Dr. R. S. Thornhill at Cambridge, largely unpublished, have shown that, if such disturbances be avoided, the water-line will remain immune for long periods (60–90 days). Zinc is more liable to attack at the water-line than iron, while wetting of the specimen above the water-line is found to increase the chance of breakdown, as would, of course, be expected.

ULICK R. EVANS.

Metallurgical Laboratories,
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July 16.

[1] Evans, U. R., *J. Inst. Met.*, **30**, 257 (1923); Evans, U. R., and Hoar, T. P., *Proc. Roy. Soc.*, A, **137**, 343 (1932); Azar, J. N., unpublished work summarized by Evans, U. R., *J. Iron Steel Inst.*, **141**, 221*P* (1940).

[2] Bengough, G. D., and Wormwell, F., *Proc. Roy. Soc.*, A, **140**, 402 (1933); *Chem. Ind.*, 262 (1936).

[3] Evans, U. R., Mears, R. B., and Queneau, P., *Engineering*, **136**, 689 (1933); Evans, U. R., and Mears, R. B., *Proc. Roy. Soc.*, A, **146**, 164 (1934); *Trans. Faraday Soc.*, **31**, 539 (1935).

[4] Evans, U. R., Bannister, L. C., and Britton, S. C., *Proc. Roy. Soc.*, A, **131**, 366 (1931); Evans, U. R., and Hoar, T. P., *Proc. Roy. Soc.*, A, **137**, 358 (1932).

Determination of Absolute from Relative X-Ray Intensity Data

IN the 'absolute measurements' of X-ray intensities of reflexions from a crystal, a measurement must be made of the monochromatic radiation incident upon the crystal in one second[1], which, though it can be done fairly conveniently by the ionization chamber method[2], requires a complicated design of apparatus in the use of the rotation photographic method. I have considered the problem in the light of a new synthesis of X-ray data[3] and found that the absolute values of $|F(h)|^2$ may be deduced to a close approximation from the relative intensity data without any further experimental work. The derivation of the equations needed in this deduction in the case of a linear crystal is shown below.

We first assume the relative intensities are known, deduced, in the case of the photographic methods, from the total blackening of the reflexion lines or spots registered on a photographic film. We then assume that corrections are made to these observed data for the various geometrical factors such as the polarization factor, the Lorentz factor, planar factor, and so on. We further assume that we have corrected for the temperature factor. (This assumption is in general not exactly true of course.) The final result is a set of numerical values $H(h)$ which are linearly related to $|F(h)|^2$ by $H(h) = c\,|F(h)|^2$, where c is the constant multiplying factor we intend to determine. For the moment we assume c is known. Then according to Y. L. equation (5) [the notation stands for equation (5) in Yü's letter[3]] we have, $E(h) = \dfrac{H(h)}{2c\psi(j)^2}$.

If we denote $\dfrac{H(h)}{\psi(j)^2}$ by K_h for $h \neq 0$ then $E_h = \dfrac{K_h}{c}$.

We further have $E_0 = \frac{1}{2} |F(0)|^2 = \frac{1}{2} \left| \sum_{a=1}^{s} Z_a \right|^2$ (E_0 here and E_h above as well as all the other symbols not defined in this letter have the same significance as in Y.L.). Now let us combine Y.L. equation (2) and equation (4) and substitute the values of E_h and E_0 given above in the resultant equation; we then obtain

$$E(q) = \frac{1}{2} \left| \sum_{a=1}^{s} Z_a \right|^2 \sum_{\mu=0}^{N/2} a_{\mu_a} \cos 2\pi q u_\mu + \sum_{\mu=0}^{N/2} \sum_{h=1}^{\infty} a_{\mu h} \frac{K_h}{c} \cos 2\pi q u_\mu,$$

where $u_\mu = \frac{\mu}{N}$. If in the equation we replace q by an integer k and substitute $E(k)$ by $\frac{K_k}{2c}$, we have, after a transformation of terms, that,

$$c = \frac{K_k - 2 \sum_{\mu=0}^{N/2} \sum_{h=1}^{\infty} a_{\mu h} K_h \cos 2\pi k u_\mu}{\left| \sum_{a=1}^{s} Z_a \right|^2 \sum_{\mu=0}^{N/2} a_{\mu_a} \cos 2\pi k u_\mu}.$$

When $k = 0$, we have,

$$c = \frac{2 \sum_{\mu=0}^{N/2} \sum_{h=1}^{\infty} a_{\mu h} K_h}{\left| \sum_{a=1}^{s} Z_a \right|^2 \left\{ 1 - \sum_{\mu=0}^{N/2} a_{\mu_a} \right\}}.$$

Since all the quantities on the right-hand side of the last two equations are known, c can be calculated by either of the equations. Consequently the absolute values of $E(h)$ and so of $|F(h)|^2$ are determined. The method might be an approximate one in general due to the approximation involved in Y.L. equation (5). Accordingly, if the absolute values of $|F(h)|^2$ are determined by the existing experimental methods, the present method would provide a test of the approximation involved in Y.L. equation (5) for the crystal in question.

S. H. Yü.

Metals Research Institute,
National Tsing-Hua University,
Kunming, Yunnan. April 14.

[1] Bragg, W. L., "The Crystalline State", **1**, 219.
[2] Compton, A. H., *Phys. Rev.*, **9**, 29 (1917).
[3] Yü, NATURE, **149**, 638 (1942).

Yü's letter[1] on the determination of absolute from relative X-ray intensities suggests several interesting possibilities. His equations might be useful to anyone working with his synthesis[2], but it is possible to devise a method not depending on the quantities $a_{\mu h}$, which are not yet available. The ideal intensity of the hkl reflexion is given by

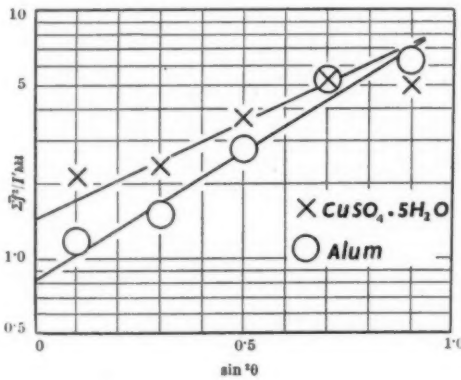
$$I_{hkl} = \sum_\alpha \sum_\beta f_\alpha f_\beta \exp\{2\pi i[h(x_\alpha - x_\beta) + k(y_\alpha - y_\beta) + l(z_\alpha - z_\beta)]\}$$
$$= \sum_\alpha f_\alpha^2 + \sum_{\alpha \neq \beta} \sum f_\alpha f_\beta \exp\{2\pi i[h(x_\alpha - x_\beta) + k(y_\alpha - y_\beta) + l(z_\alpha - z_\beta)]\}, \quad \ldots \quad (1)$$

where the f's are the structure factors and the x,y,z's the parameters of the atoms. Suppose that I_{hkl} is averaged for all reflexions in a given range of $\sin^2\theta$. If $\lambda/\sin\theta$ is small compared with the interatomic distances, the exponential terms in equation (1) will take on both positive and negative values, and their average over h,k,l will be practically zero. Then to a close approximation

$$\overline{I_{hkl}} = \sum_\alpha \overline{f_\alpha^2}, \quad \ldots \quad (2)$$

where the $\overline{f^2}$'s are the atomic structure factors for the centre of the range of $\sin^2\theta/\lambda^2$.

The experimental value of the intensity of the hkl reflexion, I'_{hkl}, will differ from I_{hkl} by two factors: a scale factor independent of θ, and the temperature factor. The ratio $\overline{I'_{hkl}}/\overline{I_{hkl}}$ for the group of reflexions will be the product of these. By dividing the reflexions into groups covering suitable ranges of $\sin^2\theta/\lambda^2$ and forming the ratio $\Sigma\overline{f^2}/\overline{I'_{hkl}}$ for each group, the factor $1/c$ required to convert I'_{hkl} to I_{hkl} will be found as a function of $\sin\theta/\lambda$. If the usual form of the temperature factor ($\exp\{-B\sin^2\theta/\lambda^2\}$) is correct, the logarithm of $1/c$ should be a linear function of $\sin^2\theta/\lambda^2$.



In normal Fourier and Patterson syntheses it is usual to work only with a zone of intensities, say, the $hk0$'s. If two atoms are close together in the projection, they ought to be counted as a single atom with atomic factor equal to the sum of their respective atomic factors. Since in general such coincidences cannot be predicted, there will be an error tending to make the value of $1/c$ too small. In special cases (for example, alum[3], in which the potassium and aluminium atoms coincide in the $hk0$ projection) certain coincidences can be predicted from the space group only, and allowed for.

To test the feasibility of the procedure, it has been applied to the $hk0$ reflexions of alum[3] and copper sulphate[4]. The ranges of $\sin^2\theta$ used were 0–0·2, 0·2–0·4, . . . 0·8–1 (copper radiation), but little reliance can be put on the average for the 0–0·2 group. The values of f were taken from the Internationale Tabellen[5]. The logarithm of $1/c$ is plotted against $\sin^2\theta$ in the graph above. The points scatter somewhat, but lie fairly well on straight lines. Since the measurements[3,4] are 'absolute', the values of $1/c$ for $\sin^2\theta = 0$ should be 1. Actually they are about 0·8 and 1·5 respectively. Without further investigation it cannot be claimed that the method is capable of high accuracy, but even that indicated should be useful in much structure work.

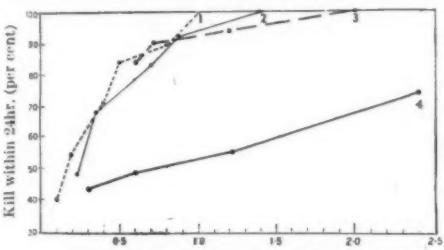
A. J. C. WILSON.

Cavendish Laboratory,
Cambridge. June 18.

[1] Yü, NATURE, **149**, 729 (1942).
[2] Yü, NATURE, **149**, 638 (1942).
[3] Lipson and Beevers, *Proc. Roy. Soc.*, A, **148**, 664 (1935).
[4] Beevers and Lipson, *Proc. Roy. Soc.*, A, **146**, 570 (1934).
[5] "Internationale Tabellen zur Bestimmung von Kristallstrukturen", **2**, 571 (Berlin: Gebrüder Borntraeger, 1935).

A New Active Constituent of Pyrethrum Flower

THE pyrethrin content as determined by the chemical process is the only factor that influences the market value of pyrethrum, as it is held that a close relationship exists between the amount of pyrethrin present and the number of insects killed. The results of our experiments (see graph) carried out with different samples of pyrethrum, clearly indicate that (1) the percentage kill of insects bears no direct relation to the amount of pyrethrin present in the samples, (2) a smaller amount of pyrethrin extracted from a sample yielding a high pyrethrin value produces better biological results than a larger amount from a poorer sample.



Pyrethrins (mgm./c.c. diluted extract)

1. PYROCIDE 20. PROPRIETARY EXTRACT.
2. PREPARED FROM *Chrysanthemum cinerariæfolium* (INDIAN); PYRETHRIN CONTENT, 0·702 PER CENT.
3. ,, ,, *C. cinerariæfolium* (JAPANESE); PYRETHRIN CONTENT, 0·602 PER CENT.
4. ,, ,, *C. roseum* (INDIAN); PYRETHRIN CONTENT, 0·247 PER CENT.

It therefore follows from the above that in addition to pyrethrins I and II, the only active constituents so far known, there exists some other toxic principle which operates in the same way as pyrethrin in the production of the characteristic effect of pyrethrum on insects. As the nature of this new substance is not known, it may be designated as *X*. Why a smaller quantity of pyrethrin from a rich sample of pyrethrum produces better results than even a considerably larger quantity extracted from poorer samples may be explained by the fact that the active principle *X* exists in larger amount in the former than in the latter. This also accounts for the fact that a sample of pyrethrum possessing a high pyrethrin content yields better results than another with a lower pyrethrin value.

The results also suggest that the action of the new active principle *X* is not complementary to that of pyrethrin. It is probably an independent body, the growth of which in the flowers is regulated by the same factors as pyrethrin. Both, however, are soluble in kerosene.

The present work confirms our previous finding that kerosene extract prepared from the sun-dried residue left after the removal of pyrethrins I and II from a sample of pyrethrum flowers was found still active, and this led us to arrive at the tentative conclusion that in addition to pyrethrins there were other substances which were responsible for the destruction of insects and mosquito larvæ[1].

For each experiment a hundred flies, *Musca vicina*, all three to five days old, were employed, and our results represent the average of three observations carried out on successive days. The extracts were prepared for us according to the method used for chemical assay[2]. Kerosene was used as the vehicle for diluting the extract; this in itself has not been shown to possess any appreciable insecticidal properties.

The biological variation from the mean calculated from the results of three experiments ranged between 0 and 5 per cent in each instance.

Sample (1) is a commercial standardized preparation known as Pyrocide 20.

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Department of Medical Entomology,
School of Tropical Medicine,
Calcutta. April 21.

[1] Chopra, Roy and Ghosh, *J. Mal. Inst. Ind.*, **3**, 457 (1940).
[2] Lahiri, Ghosh and Chopra, *J Amer. Pharm. Assoc.*, **30**, 72 (1941).

A Recording Paper for Field and General Use

AUTOGRAPHIC instruments frequently use a recording paper coated with metallic oxides, on which a black trace is made by a style of copper or silver. While this paper is very convenient, it is not always suitable for field work. To meet the need for a recording paper that would withstand difficult conditions, a special paper has been developed at the Road Research Laboratory. The following details of the process are given in the hope that they may be of use to other workers.

A good quality thin tracing paper, having a smooth surface, is damped and lapped round the recorder drum, the overlap of the paper being fixed with ordinary gum. On drying, the paper shrinks and stretches tightly round the drum, giving a very firm surface with a scarcely perceptible joint. When dry, the paper is spray-painted with a suspension of zinc oxide in dilute cellulose varnish. The resulting surface is pure white in colour, with a smooth matt texture, and on it a hard bronze style makes an excellent black trace.

Drums so coated can be used up to a very high writing speed; a time-base of 300 in. per sec. has been used with complete success, and there is no indication that higher speeds could not be used. The paper surface is strong enough to withstand the style running many times over the same base line without the paper being cut, and the hard bronze style keeps its point very satisfactorily.

In particular, the paper is waterproof, in the sense that rain falling on the paper can be wiped off without affecting the surface. Indeed, records have sometimes been irrecoverable for some days after use, during which period they have been exposed to rain with no damage to the record. It is not desirable to expose the paper to prolonged damp before use, as ultimately the paper absorbs water, swells, and becomes slack on the drum, but an accidental splash does not matter. When damp conditions are likely to be met, the cellulose coating should be rather thicker than is normally used.

After use, the paper is removed from the drum by slitting it with a sharp knife: if the paper is then dry it will come away cleanly from the drum. The resulting records are fully permanent, and will withstand any ordinary handling, filing, etc. They can be written on with pen or pencil, and pencil marks can be erased as with any ordinary paper. The sheets are also sufficiently translucent to allow records to be compared by superposition, or to be measured by putting them over a sheet of strongly marked squared paper.

There is nothing difficult in the preparation of the paper, and a stock of ready-coated drums is a convenience, especially in the rough conditions of large-scale field tests.

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July 14.

The Polygene Concept

DR. PAUL G. 'ESPINASSE voices[1] misgivings about Mather's polygenic interpretation of selection which are, no doubt, widely felt outside the field of genetics. Each step in the development of genetic theory has given rise to such misgivings. Mather's explanations of his experiments, however, cannot be dismissed as epicyclic (nor for that matter will epicyclic explanations always explain anything). The assumption of polygenes is not even *a priori*. Hereditary properties are very generally known to show mutation, segregation and linkage. Polygenic variability depends on units, factors, elements, or (why not say it?) genes, which show mutation, segregation and linkage. These are indeed the required criteria. Many important hereditary properties are not individually identifiable. There are hundreds of incompatibility or self-sterility genes known in flowering plants and fungi. Sometimes these genes include only one allelomorphic series, sometimes two. In either case they are not individually but only relationally identifiable. Yet for twenty years no one has questioned the validity of the gene explanations of Kniep and East.

Again, in Œnothera there are complex gene differences the linkage, segregation and mutation of which are responsible for the hereditary properties of a large group of species and their hybrids. The genes responsible are located in particular parts of particular chromosomes. No one has estimated their number. No one probably will do so. But that the particles responsible have the chemical constitution, mechanical arrangement and physiological properties recognized in the genes of maize and Drosophila no one doubts.

The "real methodological difficulty" for Dr. 'Espinasse depends on the polygenes being too small in their individual effects to be separated and counted. This was held to be the real methodological difficulty in the way of accepting the atomic theory. It is the kind of difficulty that arises in the making of any hypothesis. It is a difficulty, however, which is habitually relieved by new discoveries. In this case already it can be slightly relieved. Most plants and animals contain in their chromosomes, apart from the ordinary material which is recognized as carrying the genes of large effect, other parts carrying no identifiable genes. These parts were formerly regarded as inert. Yet they have recently been shown[2] to exercise an effect on cell processes which influences the character of the whole organism. Their influence is so profound as to be less specific than that of the active genes. They are therefore less readily identifiable. None the less they are the demonstrable object of selection, both natural and artificial; and the numbers of genes in them are quite beyond estimation. Indeed there cannot be much doubt that they fulfil the conditions required by Mather's polygenes. This again is a hypothesis. But it is not an epicyclic hypothesis. On the contrary, it shows that coherence with experimental breeding and with chromosome study which is characteristic of genetic theory.

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[1] NATURE, **149**, 732 (1942).
[2] NATURE, **149**, 66 (1942).

MAY I suggest that the difficulty in respect of the polygene concept, familiar to earlier writers as the theory of multiple or cumulative factors, put forward by Dr. 'Espinasse[1], is rather logical in character than, as he describes it, methodological. In simple genetics the breeding behaviour exhibited in a group of experiments is 'explained' by a few Mendelian factors, the effects of which are sufficiently different for them to be recognized individually. In the case of many quantitative characters the phenomena of heredity open to observation may be 'explained' on the supposition that there are many Mendelian factors having similar, though not necessarily identical effects. In 1918 I was able to show that such a view accorded well with every feature of the phenomena brought to light by the biometrical study of inheritance in man[2]. When Dr. 'Espinasse asks "What is the scientific value of such an explanation?", the answer is the same in both the cases. Its value lies in its ability to predict the results of future breeding operations.

It is revealing to read: "In order to account for the behaviour in heredity of a variable quality . . . it seems now to be assumed that its behaviour depends upon the distribution of members of several such pairs, of which there may be no independent knowledge". Though the notion is new to him, Dr. 'Espinasse might at least recognize its obviousness and simplicity. The factors postulated are inherited by the same rules which factors investigated independently have been shown to exhibit. They are supposed to be linked in the same systems which have been discovered by the study of individual factors. Does Dr. 'Espinasse find a 'methodological' difficulty in geology when the existence of granite on the surface of the earth leads to the supposition that a similar substance may exist in regions inaccessible to direct observation?

One of the features to which I directed attention in 1918 is that the hereditary behaviour to be observed does not become more complex as the number of factors is increased. Just as, in the theory of gases the gross behaviour of a molecular aggregate does not depend upon any accidental and momentary configuration of the particles, with its endless complexity, but upon a few constant statistical properties of the aggregate, so it is with the simultaneous action of numerous Mendelian factors. Dr. 'Espinasse's suggestion: "If the number of such pairs invoked as relevant be increased just until they are sufficient to explain the observations", shows a total misapprehension of this situation. In studying the selective response in lint-length of cultivated cottons Panse, for example, has recently exhibited[3] parallel models, some using few (only three) factors, and others an infinite series. The number of factors involved is in fact one of the least influential features of the systems investigated.

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Harpenden, Herts.

[1] 'Espinasse, P. G., NATURE, **149**, 732 (1942).
[2] Fisher, R. A., *Trans. Roy. Soc. Edin.*, **52**, 399 (1918).
[3] Panse, V. G., *Ann. Eugen.*, **10**, 76–105 (1940).

RECENT ADVANCES IN AMERICAN ARCHÆOLOGY

THE sessions of the first day of the annual general meeting of the American Philosophical Society, held in the Hall of the Society, Independence Square, Philadelphia, during April 23–25, were devoted to a symposium on "Recent Advances in American Archæology", and were followed by a lecture in the evening by Dr. Sylvanus G. Morley on the work of the Carnegie Institution in Central America and Mexico, to whom the Society's Franklin Medal has been awarded for his investigations in this field.

"The Problem of the Eskimo" was discussed in the opening paper by Dr. Alěs Hrdlička. The Eskimo are one of the largest, still most pure-blooded, and in many respects among the most important of American native groups, and deserve, therefore, all possible scientific attention. They are, moreover, in the flux of a rapid change towards civilization, and all this carries with it, which by itself offers a great interest to the scientific observer. The anthropological problem of the Eskimo is not simple. It comprises the questions of their racial identity; their place of origin and antiquity; their relation to the Indian; the modes of their immigration; the reasons of their peculiar spread; the limits of this spread; the cultural history of the people; their physical characteristics; their homogeneity or heterogeneity; their relation to the Asiatic peoples; their relation to the south-western Alaskan groups; the present status of and changes in the Eskimo; their admixture with Indians, with Whites, with Negro; their diseases; and their prospects, demographic and anthropological, for the future.

"The Aztecs: their Cultural and Historical Position in Middle American Archæology" was the subject of a paper by Dr. George C. Vaillant. For various reason the Aztecs occupy an important place in Middle American archæology. They had one of the highest cultures in the Americas at the time of the Conquest. They have been more carefully observed since the sixteenth century than any other tribe in North or South America. In addition, they had their own manuscript records, which are capable of historical interpretation. Thus, from the point of view of achieving the full picture of American Indian civilization, we have more information than for any other part of the Americas.

There seems to have been in the Middle American area first an era, indefinite as yet archæologically, whereby peoples took their first steps toward agriculture. Next follows a long period of sedentary village life based on agriculture. Then came the development of the theocratic State involving pyramid construction, establishment of a pantheon, the creation of some sort of a calendar system and some type of writing. These traits were developed by a number of tribes who were completely independent racially and linguistically, and there was a constant type differentiation in style and form in their handling of these generalized problems. Apparently, about the eleventh century these ritualistic civilizations began to collapse, partly from internal causes and partly from external pressure of a military and political nature. The effect of this change is manifested by the presence of Nahua-speaking people diffused over a wide area. As a corollary to this expansion in Middle America we have in South America the creation of the Inca Empire, the only true empire in the Americas, and in North America evidence of tribal movements strongly affecting the population of the south-western United States and the south-east as well.

Prof. Harold S. Colton presented a "Reconstruction of Anasazi History". This group of prehistoric Indians, who lived on the plateau areas in the South-west, can be divided into six or more separate tribes, each with an independent history. Although all these tribes have much in common, they can be distinguished one from another over a long period of years.

In reconstructing their history, tree-ring studies have furnished a time-scale, calibrated in terms of our own era. Besides this we must recognize the following factors: (1) the persistence of certain culture traits, and (2) the diffusion of others. We must recognize and study the properties of frontiers between the Anasazi tribes themselves and between the Anasazi tribes and the surrounding non-Anasazi neighbours. We must recognize crises in the history of each, dominance or climax as well as the influence of trade and trade routes. From these factors it is possible to reconstruct the history of each separate cultural unit.

Dr. Dorothy Cross described "The Effect of the Abbott Farm on Eastern Chronology". These excavations on the Abbott Farm, along the Delaware River bluff, south of Trenton, New Jersey, have disproved former theories of both a pre-glacial and a clearly defined 'argillite' culture. The artefacts obtained from excavations on the forty-five to seventy-five foot bluff had a vertical distribution similar to that noted on most New Jersey sites, namely, they fall into a normal frequency curve with the peak three to four inches below the 10-in. plough line. No definite levels were discernible, and cultural changes were meagre.

The excavations on the marshy lowlands at the foot of the bluff disclosed three former humus layers, the lowermost 9 ft. from the present surface, separated by virtually sterile layers of sand and gravel. The culture in each humus layer was similar and the human remains showed the same physical type. Pottery of the same texture, surface finish, tempering and decoration was found in all humus layers in the lowlands and at all cultural-bearing depths on the bluff. Although argillite and shale were the most used materials, they were not concentrated at any given depth. Blanks, which were formerly confused with 'palæoliths', were found at all depths and of all materials, including jasper.

Traits diagnostic of certain foci and aspects in regions adjacent to New Jersey are well integrated at the Abbott Farm, and lack the chronological implications they appear to have elsewhere. The Abbott Farm has produced more than a hundred thousand artefacts and potsherds, which, by substantially eradicating negative evidence, may partially account for the uniformity of culture.

Prof. Fay-Cooper Cole's paper, "Chronology in the Middle West", dealt first with the chaotic condition of Mid-Western archæology which existed until a few years ago. Eventually several institutions collaborated in a series of field and laboratory conferences to discuss methods of excavation and fundamental problems, and out of this came the Mid-Western taxonomic system which has brought about systematic treatment of the data.

At present two widespread patterns of culture, with many subdivisions, are recognized and their

chronological order determined. Hints of an early pre-pottery horizon occur, then follows an early Woodland, out of which several distinct divisions emerge. Such names as Red Ochre, Black Sand, Central Basin, Lake Michigan, Baumer and Lewis stand for well-established cultural complexes which can be traced over wide areas. Into this situation came influences from the south and south-east which, meeting with Central Basin, produce the Illinois Hopewellian with its spread into Indiana and Iowa. A similar meeting with Lake Michigan appears to produce the Hopewell of Wisconsin. The early stages of the Mississippi pattern are lacking, but the late, fully developed complex with large settlements, truncated pyramids, and a great variety of cultural objects spreads over much of the Middle West. Upper Mississippi extends its influence over northern Illinois, Wisconsin and Iowa, where it continues into historic times as the Winnebago and Ioway Sioux.

Shortly before the coming of the Whites, great cultural shifts take place and the northern Woodland groups, known to the early settlers, become established.

Prof. Willian S. Webb described "Early Horizons in the Southeast". Artefacts typologically similar to those from the Lindenmeir site have been found *in situ* at two sites in Kentucky and in Georgia. These suggest the possibility of a South-eastern Fulsom cultural horizon. While these sites are non-pottery, and all evidence points to considerable antiquity, the absence of stratigraphy leaves their chronological placement undetermined. Deep shell middens on the banks of large streams in Alabama and Kentucky show long occupancy, and a distinctive cultural complex. Sites are either entirely non-pottery or have a non-pottery component at their base. Stratigraphy demonstrates that in Alabama this complex antedates all pottery-making peoples. The material culture complex is dominated by the use of worked bone and antler, and shows the atlatl in use in a variety of forms. Polished stone atlatl weight and large flint projected point are common.

T. M. N. Lewis discussed "Late Horizons in the Southeast". Interpretation of the late archæological horizons in the South-east, he said, is obliged to take into consideration the historical facts concerning the identities, characteristics and distributions of the various Indian tribes at the time of White contact. Even more important than the internal developments of the tribal cultures is the unravelling of the acculturation processes which were operative. The earlier occupants did not always disappear from the region, but tended to be displaced and their cultures modified by contacts with later peoples. Desirable village sites were successively occupied by peoples of divergent cultures and the remains are frequently inextricably mixed notwithstanding the fact that desposits are occasionally deep and stratified.

Three of the widely recognized archæological manifestations of the eastern United States, the Woodland, Hopewellian and Mississippi complexes, are not only involved in the longer perspective of south-eastern prehistory but also represent the important factors in the late horizons. Other more restricted south-eastern elements are also included in the late manifestations of this cultural province.

The chronological sequences of the various cultural entities which have been established in the areas of the South-east, where intensive investigation has recently been carried on, can be reconciled in a general way. These sequences are not always the same throughout this large region, but the major influences are discernible in comparable chronological relationships and the interpretations can be co-ordinated.

The major impediment in applying the Midwestern Taxonomic System to the South-east lies not in the principles of the system but in the fact that the determinants for the broader categories, such as pattern, phase and aspect, were set up largely on the basis of northern data or upon limited data from the South-east.

The most positive ethnic identifications of the proto-historic and historic cultures of the South-east have been confined mostly to the tribal groups of the Muskoghean linguistic stock, but there are some more speculative recognitions of cultures of ethnic groups, such as the Yuchi, Cherokee and Shawnee, which give promise of eventual substantiation and acceptance.

Frank C. Hibben read a paper describing "Discoveries in Sandia Cave and Early Horizons in the Southwest". Sandia Cave lies on the east side of the Sandia Mts., some eighteen miles from the town of Albuquerque, New Mexico. Excavations in the Sandia Cave were carried on during 1936–39 and also included tests and explorations in several other cave sites in the Sandia-Manzano area.

The chief importance of the Sandia site lies in its stratigraphy, which involves an already well-established horizon of early man, the Folsom. The stratigraphic sequence of the cave involves, from the most recent to the earliest remains, a superimposed stratum of dust and bat accumulation containing in its topmost accumulation a 3–6 in. layer of solidified calcium carbonate; below the calcium carbonate crust a considerable layer of varying thickness consisting of charcoal fragments, flint chips, bones, angular rock fragments, and other cave debris with intermixed Folsom points, gravers, scrapers, and other implements, indicative of a Folsom horizon; below this Folsom cave occupation, a stratum of sterile yellow ochre; beneath the yellow ochre, another cave floor with associated debris of bones, charcoal, and implements of a shape and manufacture different from the superimposed Folsom varieties, these earlier and lower placed points being distinguished by a side shoulder and rougher chipping as compared with the Folsom, and called, from the cave site, 'Sandia points'; below the Sandia level another sterile layer to bedrock. This stratigraphy is supported by the included mammal remains, which indicate that the Sandia and Folsom levels are late Pleistocene in date, as all large forms are of extinct species, and that the stratum above the calcium carbonate is recent, since only the sloth is found in the lowermost portion of this level.

In another cave in the same region, Manzano Cave, additional stratified discoveries were made which added to the above sequence. Projectile points and bola weights were found in a stratum corresponding to the Early Recent of the Sandia Cave. These projectile points are similar to those of Gypsum Cave. On a basis of these discoveries, then, the following sequence of events concerning the history of the Palæo-Indian may be proposed:

Manzano: Gypsum Cave, camel and sloth association; Early Recent.
Wet period: end of Pleistocene.
Folsom: bison, horse, mammoth association: end Pleistocene or Early Recent.
Wet period: late Pleistocene.
Sandia: bison, horse, mammoth and mastodon association; late Pleistocene.

In a paper entitled "A Possible Cochise-Mogollon-Hohokam Sequence", Emil W. Haury pointed out that in recent years American archæologists have established several culture groups on hunting and food-gathering planes of subsistence which have been assigned to time levels ranging from about 10,000 to 20,000 or more years ago. Folsom, Sandia, Clear Creek, Gypsum Cave, Lake Mohave, and Cochise remains are a few of the manifestations. While the tendency in these studies has been to push the history of man back into antiquity, refined dating methods, and particularly the development of the tree-ring dating technique, have caused the chronologies of the 'higher', or agricultural people, as for example the Basketmaker-Pueblo, to be revised upward and shortened over previous estimates. An ever-widening gap between the early and the late groups has consequently resulted and doubts have been expressed as to the possibility of establishing any continuity between the two.

An exception to this situation may be found in the southern portion of the South-west, where the Cochise Culture existed. This culture, as reported by Sayles and Antevs, has three recognizable stages, the earliest of which is dated at more than 10,000 years ago, and the latest stage ending approximately 500 B.C., or just before the appearance of pottery and presumably agriculture.

Combining the evidence which has been gathered from open sites and from Ventana Cave, recently excavated by the Arizona State Museum, there are grounds for postulating that the agricultural and pottery-making Hohokam and the Mogollon Cultures have roots which dip into the Cochise complex. Expressed in other terms, the addition of pottery, agriculture, and possibly a few minor traits, to the lithic industry of the San Pedro Stage of the Cochise Culture, produced the earliest known ceramic horizons of the Hohokam and the Mogollon. The early phases of these two groups have much in common, especially ceramically, and it would appear that a common parentage may be claimed.

In a discussion of "Folsom and Yuma Problems", Edgar B. Howard said that satisfactory proof is still lacking to give more than a relative dating for the length of time man has been on the American Continent. Extreme estimates range from about 2000 B.C. to 70,000 years or more ago. The Folsom problem, then, is only part of the broader one dealing with answers to the questions: How long has man been in America? Where did he come from? How did he get there? Before any attempt can be made to answer these questions it is essential to have some understanding of what constitutes a Folsom point and a Yuma point, since reference to these types of spear points appears in every discussion of early man in America.

No matter from what angle the question is approached, one becomes aware of its complexity. It is made up of what appears to be a number of unrelated problems, which, in reality, are parts of the same broad puzzle, namely, man. Many secrets, so far as America is concerned, are locked up in Siberia. Until we know more of the glacial geology, anthropology, and archæology of this region, we cannot hope to answer, with any degree of satisfaction, questions such as that relating to migration routes, the culture stage reached by these early wanderers, and many others which are necessary as a foundation to a real understanding of American prehistory.

L. S. Cressman described "Cave and Lake Bed Cultures of South Central Oregon in the Northern Great Basin": Cave deposits of camp debris separated by pumice have been excavated in south central Oregon. The pumice, which came from the eruptions of Mt. Mazama and formed Crater Lake in the Cascade Mountains, has been given an age of not less than 5,000 and not more than 10,000 years by Dr. Howel Williams. The cultural material above does not differ from that below the pumice except in quality. Caves east of the pumice area contain the same kind of material.

These eastern Oregon caves show the transition from the atlatl or spear thrower to the bow and arrow. Fine twined basketry, the most conspicuous type of article found in the caves, must have been brought in by migratory peoples, for it appears completely developed immediately following a period without basketry. In the eastern caves near the end of the occupation were found a few fragments of coiled basketry. Well beneath the pumice in one of the stratified caves were found chipped obsidian tools, bones of horse, camel and several other genera along with the camp fires used to cook the flesh of these animals.

The bed of an ancient lake has given us three horizons: (1) the oldest, when horse, camel and some elephant-like animal slaked their thirst at the dwindling water holes and were hunted by man; (2) a second, about the shore-line when the lake was beginning to refill about 4,000 years ago; and (3) the historic Modoc culture about the modern shore line, dating from about the beginning of the Christian era.

Analysis of the basketry shows relationship with the culture of the earliest period of Lovelock Cave and the Humboldt Caves of the same region; technological details suggest a relationship with the Basketmaker cultures of the South-west. L-shaped awls made from the scapula of large animals suggest the Oregon - early Lovelock - Basketmaker relationship. Given the lapse of time indicated by the pumice, the Oregon material is antecedent, and perhaps ancestrally related, to certain elements of the classical Basketmaker culture.

In a paper on the "Archæology of the Andean Field", Wendell Cl. Bennett said that Peruvian archæological chronology has long been based on the sequences established by the extensive work of Max Uhle. Only recently has the importance of the Chavin style and culture been demonstrated, although it has long been recognized by such leading Peruvian archæologists as Julio C. Tello and Rafael Larco Hoyle. An examination of the site of Chavin on the River Marañón in the north highlands of Peru reveals a distinctive architectural style, a stone carving style based on the feline concept, and a simple monochrome incised ceramic style. Discoveries on the coast of Peru show similar ceramics at early sites in Ancon and Supe valleys, and more ornate ceramics with incised and relief designs similar to those on Chavin stone carving at many other sites in such valleys as Casma, Nepena, Viru, Chicama, Lambayeque and Piura. These discoveries show that the coast Chavin period is associated with buildings made of rough stone and conical adobes, sometimes with plastered walls elaborately carved and painted. Flexed burials covered with red paint were found by Larco in Chicama valley associated with the Chavin ceramics plus objects of gold, bone, shell and stone all incised or carved with the typical designs.

A review of the above evidence indicates that:

(1) Chavin is older than any period yet discovered on the north coast of Peru; (2) as a style Chavin has a wide distribution throughout Peru and bids fair to become a third pan-Peruvian period along with the Tiahuanaco and Inca periods; (3) the ramification of styles pertaining to Chavin together with its antiquity and distribution make it one of the basic factors in the revised chronology.

CAUSES OF INTERTIDAL ZONATION

ON June 18 and July 9, the Linnean Society of London, under the chairmanship of the president, Dr. E. S. Russell, discussed the causes of the intertidal zonation of plants and animals. The discussion was opened by Prof. T. A. Stephenson, who attempted to elucidate the reasons for both the vertical zonation and the horizontal distribution round the coast of the common intertidal organisms of South Africa, as observed by himself and his collaborators during the years 1931–40 (see NATURE, 143, 503; 1939). In his introductory remarks Prof. Stephenson stressed the importance of field observation, as well as that of laboratory and field experiment, for the interpretation of intertidal phenomena, and pointed out also that since much of what is observed between tidemarks is evidently due to the operation of complexes of factors, it is doubtful whether much is to be gained by too great insistence on the part played by each individual factor in a complex. After a review of a number of the factors which may be expected to be concerned in intertidal processes (nature of substratum, desiccation, temperature, wave-action, feeding-habits, competition for food and space, salinity, submergence and emergence, oxygen, carbon dioxide and hydrogen ion concentration, nutritive salts, light and taxes) the conclusion was reached that there is scarcely any factor the effect of which between tidemarks can be imagined which is unable to produce *some* effect on zonation, although certain factors are undoubtedly more important than others; and that the effect of some factors is much more local than that of others. It was further concluded that the principal agent responsible for the *horizontal distribution* of organisms around South Africa is sea temperature; that for *zonation on open rock* the controllers of primary importance are degree of exposure to the desiccation-heat-light complex acting together with degree of exposure to wave action; that for *zonation in rock-pools* the leading factors appear to be variations in temperature and salinity; and that *light* is probably more generally effective in determining the segregation of shade-loving from surface species than in contributing to the actual zonation. In all cases there are subsidiary effects due to causes other than the major ones.

Dr. J. S. Colman next contributed an account of some interesting intertidal enigmas, contenting himself with describing certain distributions for which explanation is urgently needed but which, up to the present time, remain quite unexplained. This contribution brought out very clearly what was recognized by all speakers, that although papers such as those contributed to this discussion introduce "an element of reason into a distribution which could previously best be described as orderly but irrational" (Dr. Colman), yet the rationalizations hitherto attempted are undoubtedly too simple and very far from being able to explain all the facts observed.

The third speaker, Dr. E. M. Delf, dealt with the significance of the exposure factor in relation to zonation, enumerating its components: this contribution was in line with those of earlier speakers, and added reinforcement by means of a different selection of details, bringing out once again the fact that intertidal phenomena depend on complexes of interacting factors.

The meeting was then thrown open for discussion, and Dr. G. P. Bidder contributed a number of interesting examples taken from among the sponges, some of which threw additional light on points raised by previous speakers, while others introduced further features of intertidal life which had not previously been dealt with. Prof. L. Newton followed with some comments referring to algæ, noting particularly that, at least in Britain, the nature of the substratum may become important locally as a factor determining the nature of the flora; and that in past discussions on algal zonation the effects of light had probably been overestimated. Prof. Stephenson, Dr. Colman and Dr. Delf replied briefly to these comments.

On the resumption of the discussion on July 9, Mr. A. D. Cotton, in the unavoidable absence of the author, read a paper by Dr. V. J. Chapman on the horizontal distribution and vertical zonation of marine algæ on rocky shores. Dr. Chapman limited his attention to the common Fucoids. He found himself in agreement with Prof. Stephenson's opening remarks as to the validity of field-work as well as of experiments in the search for the causes of zonation, and also agreed that, of the factors controlling horizontal distribution, temperature appears to be the leading one. Passing to the question of critical levels between tidemarks, Dr. Chapman compared the work of several authors and synthesized their observations for five different localities, concluding that the existence of certain critical levels may be a reality despite undoubted local variations. In an assessment of the various factors the operation of which will render one level more critical than another, the effects were reviewed of mechanical exposure, substratum, submergence and emergence, exposure to air, temperature, illumination, salinity, gaseous exchange and nutrient salts, desiccation and biota. These factors were classified as *causal*, *presence or absence* and *modifying* factors respectively. Lastly Dr. Chapman considered the relation of algal zones to tidal levels in the light of the foregoing review, and pointed out lines along which further progress might be made in the search for the causes of zonation. Mr. Cotton, in reading the paper, added comments of his own referring to matters of detail, but no further discussion took place.

Regarded purely as a verbal discussion, this symposium was disappointing, because, as the result of an unfortunate combination of circumstances, several of those interested were unable to attend the second session of the discussion, and some of them not even the first. At the same time there was a considerable measure of agreement between the views expressed, and the subject was seen to have been somewhat clarified during recent years in spite of universal recognition of its complex nature and of the need for extensive future investigation; and the papers contributed should, when printed in the Society's *Proceedings*, form a valuable reservoir of ideas for the further development of the subject.

T. A. STEPHENSON.

JUNCTION LINE FILTERS

TWO articles on these filters by F. A. Hinshaw and J. O. Israel respectively (*Bell Lab. Rec.*, **20**, No. 9, May 1942) describe their design and applications in communication circuits. Open-wire lines brought into repeater stations or toll offices located in densely populated areas are usually terminated at the outskirts of the area, and the circuits are brought to the office through cables. These cables are usually loaded so as to improve the impedance match between the open-wire line and the cable and to reduce the loss in the cable. At repeater stations and terminals the separation of the carrier and voice circuits is ordinarily accomplished by filters at the office end of the cable. It is not feasible to use the office filters to separate the carrier and voice circuits at the terminal pole to avoid the necessity of carrier loading. Moreover, difficulties would be encountered in obtaining a suitable balance at the voice repeater because of the imperfect impedance match of filter and cable, and of the length of cable between filter and repeater, which would be different for each installation.

These difficulties have been overcome by the development of the junction line filter and its associated networks, the principal duty of which is to separate carrier and voice currents. This separation is accomplished by two single-section filters, a low-pass and a high-pass. As soon as these filters are introduced into the line, however, several other factors become important, the most outstanding of which are longitudinal balance, reflection coefficient, and modulation. Two junction filters are available. In one, the low-frequency channel is wide enough for a 5 kc. programme circuit, while the high-frequency channel is suitable for open-wire carrier telephone systems operating at frequencies as low as 6·5 kc. In the other, the low-frequency channel is wide enough for voice frequencies up to 3 kc., while the high-frequency channel passes frequencies as low as 4·8 kc.

By the use of these junction filters, carrier loading of the longer entrance cable is unnecessary; the voice frequencies are brought in over a voice-loaded pair, and the carrier frequencies over a non-loaded pair. Certain additional equipment is also required. An impedance-matching network on the cable side of the high-pass filter corrects the impedance mismatch that would otherwise exist at carrier frequencies between the filter and non-loaded cable pair. At the office end of the carrier pair a transformer network transforms the relatively low impedance of the non-loaded cable pair to the office impedance. Since with this arrangement the office filters are no longer connected in parallel, compensating networks are required to simulate the impedance effect of one filter on the other.

The insertion of the filter at the terminal pole, however, introduces an impedance irregularity in the voice frequency pair and upsets the balance at the hybrid coil. To maintain a suitable balance, therefore, the effect of the filter and the length of cable must be duplicated in the balancing network. Where loaded entrance cable is connected directly to the open-wire line, the balancing network includes a section to simulate the low-pass filter, a section to simulate the composite set, and a two-terminal network to simulate the impedance of the open-wire line or loaded entrance cable. Where the junction line filter is employed, however, the network must include other elements to simulate both the filter and the particular length of entrance cable involved.

For simulating the transmission characteristics of the entrance cable, which may be of different length for each installation, a series of cable-simulating units has been provided. Three types of simulating networks have been developed and with a suitable combination of them and a suitable amount of building-out, any length of entrance cable can be simulated. Terminal sections are built up in a similar manner to simulate the characteristics of the terminal section.

The ability of the junction filter to operate out-of-doors under all kinds of weather conditions and its comparatively low cost make it an economical substitute for carrier loading on the longer entrance cables. While the junction filter gives transmission results sufficiently satisfactory to justify its use, it does not provide as good overall impedance matching as carrier loading, and it increases rather than decreases entrance cable losses.

'TACK' AND STICKY MATERIALS

AN informal discussion on the subject of 'tack' and sticky materials, arranged jointly by the British Rheologists' Club and the Printing and Allied Trades Research Association, was held at St. Bride Institute, Fleet Street, London, on June 10. Dr. G. L. Riddell presided.

In his introductory remarks, Dr. V. G. W. Harrison said that the purpose of the meeting was to bring together those interested in 'sticky' materials. It is still uncertain what physical properties produce 'stickiness' and it was hoped that as a result of the meeting ideas would be clarified and perhaps some preliminary investigations could be started. Those present represented a wide range of interests, and it was generally felt that an informal meeting of this kind was long overdue.

Mr. C. C. Mill opened the discussion. 'Tack', he said, is a common term in the printing and allied trades. It means, in a general way, 'stickiness', but it has no precise definition and is usually estimated by a 'finger test'. A certain amount of 'tack' is considered essential in printing ink, and it is thus desirable to have some means of defining and measuring this property. Various attempts have already been made. Recently Green (*Ind. and Eng. Chem.* Anal. Edit., 13, 632; 1941) has described a method which attempts to reproduce the finger test. He arrives at an equation in which the time taken to move a mechanical 'finger' a fixed distance is a function of the geometry of the 'finger' system and the viscosity of the material tested.

If Green was really measuring 'tack', Mr. Mill continued, then this property is identical with viscosity, and all liquids of the same viscosity should have the same 'tack', which is contrary to the usual belief. It seems probable that Green over-simplified the problem. In addition to the stress required to maintain viscous flow between the separating surfaces, a further force is required to break the film. This force is dependent on the cohesive and adhesive properties of the liquid, and thus varies from one liquid to another. Another possible factor causing 'tack' is suggested by Maxwell's conception of 'relaxation time'. With an ideal liquid the stress produced by a given strain is rapidly dissipated, but in

many real liquids the time taken is much longer; consequently these liquids show elastic properties under certain conditions, and it may be found that 'tacky' materials belong to this class.

If it could be established, Mr. Mill concluded, that 'tack' is due to some combination of fundamental physical properties, it would become possible to devise standard methods of measuring it; but until something more definite is known, it does not seem worth while to continue experimenting along empirical lines. Probably the best way of solving the problem would be to get it taken up as a fundamental research by one of the universities or a research association. Some progress might be made in the meantime by preparing a set of samples with (so far as possible) known physical properties, and submitting these to 'practical men' in the industries concerned for grading according to their own crude tests for 'tack'.

In a written contribution, Mr. J. Pryce Jones said there is slight evidence that 'tacky' liquids possess surface viscosity. He adduced evidence to show that there is no direct relationship between 'tack' and viscosity as measured by a falling ball method. Dr. E. Mardles wrote to say that he has found a fairly close correlation between 'pull resistance' on a flat 'finger' and yield value as measured on a type of Stormer viscometer.

In the general discussion, Dr. G. W. Scott Blair said that 'tack', like 'firmness', may prove to be a *Gestalt*, that is, may display properties other than can be derived from the parts in summation. The terms in such a combination do not always have whole number dimensional exponents; the form of the expression depends on the *Gestalt*, which is itself influenced by the training of the person estimating the property. Usually only one of the terms in the combination (for example, viscosity) is measured instrumentally, and the system is thereby oversimplified, perhaps to such an extent that misleading figures result. A general discussion on the parts which might compose this *Gestalt* followed, in which it was generally agreed that adhesion of the liquid to the testing surface is essential. Fairly high cohesion and viscosity are also important. The 'tack' of some materials (for example, sodium alginate solutions) seems to change with speed; with a rapid motion of the finger, there is little sticking, whereas with a slow rate of break there is distinct 'tack'. The whole problem is complicated by the fact that in the finger test the dimensions of the test sample are continually changing.

Dr. L. R. G. Treloar said that most *spinnbar* materials appear to consist of long-chain molecules in solution or associations of particles, and drawing out these materials produces orientation of the particles, which makes the threads stronger. Small threads are stronger than large because of surface orientation effects. A general discussion followed on whether *spinnbar* materials are necessarily 'tacky', and vice versa. The consistency of potters' clay is judged by 'tack', yet it is not *spinnbar* and does not contain long-chain molecules. Is this 'tack' the same as the 'tack' of adhesives and varnishes? Dr. R. F. Bowles asked whether one can have a 'tacky' solid; if not, is there any limiting viscosity which must be exceeded before a liquid becomes 'tacky'. Mr. J. H. Dawe pointed out that on account of the rapid drying of adhesives, a time factor is important. He also finds it necessary to distinguish between the 'tack' which is just sufficient to keep a label in position on a bottle, and the 'tack' necessary to keep together two surfaces which would otherwise spring apart.

Finally, it was agreed that, as a start, the Printing and Allied Trades Research Association, in collaboration with the British Rheologists' Club, should prepare a set of samples and submit these to experts in the printing ink, paint, adhesive, clay and pottery, and plastics trades for grading. Nothing may come of the experiment, but it will serve to show whether there are any signs that general agreement can be reached, or whether the conception of 'tack' varies from one industry to another. The samples might also suggest what properties are mainly instrumental in producing 'tack'. The results of the experiment could be considered at a later meeting.

FORTHCOMING EVENTS

Saturday and Sunday, August 1-2

ASSOCIATION OF SCIENTIFIC WORKERS (IN COLLABORATION WITH THE FEDERATION OF AYRSHIRE SCIENTIFIC FILM SOCIETIES) (at Ayr).—Conference on "The Scientific Film and Scientific Film Societies".

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

GRADUATE LECTURER IN THE ENGINEERING DEPARTMENT—The Registrar, North Gloucestershire Technical College, Lansdown Road Branch, Cheltenham (August 10).

LECTURER IN MINING in the Chesterfield Technical College—The Clerk to the Governors, Technical College, Infirmary Road, Chesterfield (August 15).

LECTURESHIP IN CHEMISTRY—The Principal, Heriot-Watt College, Edinburgh (August 17).

REGIUS PROFESSOR OF ENGINEERING AT EDINBURGH UNIVERSITY—The Private Secretary, Scottish Office, Fieldon House, 10 Great College Street, London, S.W.1 (September 7).

HARRIS CHAIR OF PHYSICS, University College, Dundee—The Secretary, The University, St. Andrews (December 31).

LECTURER-IN-CHARGE OF MECHANICAL ENGINEERING—The Principal, Handsworth Technical College, Golds Hill Road, Handsworth, Birmingham 21.

VISITING LECTURER IN PSYCHOLOGY—The Secretary, King's College of Household and Social Science (University of London), c/o University College, Leicester.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Food Conditions in Europe: a Statement on the Effect of War and Blockade on People in German-controlled Countries. Pp. 12. (London: Famine Relief Committee.) 3*d*. [147

Medical Research Council. Special Report Series No. 245: Report of the Committee on Bed-Bug Infestation, 1935–1940. Pp. 64. (London: H.M. Stationery Office.) 1*s*. net. [157

Concrete Simply Explained. By Victor S. Wigmore. Pp. 48. (London: Society of Engineers.) 1*s*. 6*d*. net. [167

Relief and Reconstruction in Europe: the First Steps. An Interim Report by a Chatham House Study Group. (Post-War Problems.) Pp. 40. (London: Royal Institute of International Affairs.) 1*s*. 6*d*. net. [177

Medical Relief in Europe: Questions for Immediate Study. By Dr. Melville D. Mackenzie. (Post-War Problems.) Pp. 68. (London: Royal Institute of International Affairs.) 2*s*. net. [177

University of Bristol. The Annual Report of the Agricultural and Horticultural Research Station (the National Fruit and Cider Institute), Long Ashton, Bristol, 1941. Pp. 152. (Bristol: The University.) [207

Other Countries

U.S. Office of Education: Federal Security Agency. Education and National Defense Series, Pamphlet No. 11: Populations Adrift. Pp. v+30. (Washington, D.C.: Government Printing Office.) 15 cents. [137

U.S. Office of Education: Federal Security Agency. Education and National Defense Series, Pamphlet No. 1: Our Country's Call to Service. Pp. vi+24. (Washington, D.C.: Government Printing Office.) 15 cents. [157

National Planning Association. Planning Pamphlets, No. 11: For a Better Post-War Agriculture. Pp. 48. (Washington, D.C.: National Planning Association.) 25 cents. [207